ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY ARISING FROM THE USE OF ASBESTOS IN ONTARIO

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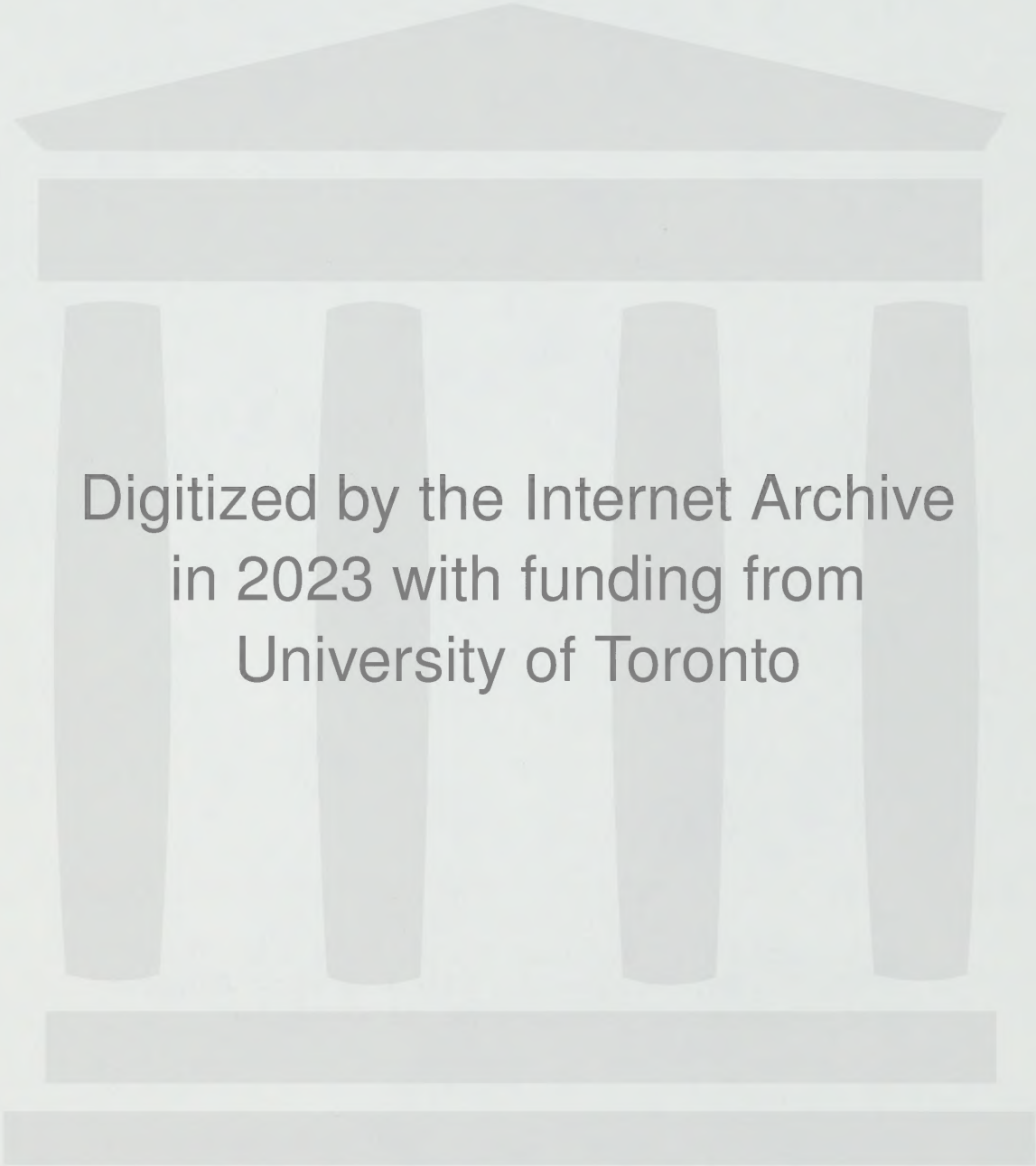
180 Dundas Street
Toronto, Ontario
Thursday,
June 18, 1981

VOLUME X



ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

ARISING FROM THE USE OF ASBESTOS IN ONTARIO

VOLUME X

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ERRATA AND ADDENDA: References to Leone Conference should read Lyon Conference

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Volume X

THE FURTHER PROCEEDINGS OF THIS INQUIRY RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Before I greet the witness, are there any matters to be raised?

MR. LASKIN: Just a couple of matters, Mr. Chairman. First of all, let me apologize to everyone for starting a half an hour late, but our witness got detained at Heathrow Airport for some considerable period of time and we thought it would be fair that we hold it down for half an hour.

Mr. McNamee is not here, but I am advised that we can proceed in his absence.

The only other matter I want to raise is, that next Monday's schedule...I think everybody already knows, but I'll announce formally...we are not going to have a hearing on Monday, and the reason for that is that Dr. Wigle, who was scheduled to come, has asked if he can defer his testimony. He had an emergency commitment within the government that he said he had to attend to, and perhaps more importantly, the study that he wished to talk to us about is not yet in the final form and he expects it will be in the final form soon.

MR. LASKIN: (cont'd.) So I have agreed to hold his testimony down and I'll reschedule it at a time convenient for everybody.

DR. DUPRE: I assume that that means, counsel, that next week the only witness will be Dr. Corbett McDonald, is that correct?

MR. LASKIN: That's the only witness is Dr. McDonald.

DR. DUPRE: Are there any other questions or points that parties wish to raise? No?

Well, may I then, please, on behalf of the Commission and I'm sure on your behalf also, greet most warmly Mr. Geoffrey Berry, who has survived work-to-rule by air traffic controllers to get to Toronto at a ghastly hour his time.

Mr. Berry, you are all the more welcome for having so generously consented to come here and give sworn, expert testimony.

MR. BERRY: Thank you, Mr. Chairman.

DR. DUPRE: Miss Kahn, would you swear in the witness, please?

MR. GEOFFREY BERRY, SWORN

EXAMINATION-IN-CHIEF BY MR. LASKIN

DR. DUPRE: Counsel?

MR. LASKIN: Mr. Chairman, Mr. Berry has kindly consented at the outset to give us an overview of his work, and some of the issues that he has been dealing with...again, with the use of slide projection equipment.

Before I come to that, perhaps I could just tell the people here Mr. Berry's qualifications, since there is no curriculum vitae attached to his brief.

His brief, perhaps, we can formally mark. It will be exhibit thirteen, for the record.

EXHIBIT # 13: The abovementioned document was then produced and marked.

MR. LASKIN: Q. Mr. Berry, I understand you have an honors degree in mathematics, and a diploma in mathematical statistics from Cambridge?

THE WITNESS: A. Yes.

Q. You are a Fellow of the Royal Statistical Society and of the Institute of Statisticians?

A. That is correct, yes.

Q. I gather for the last fifteen years you have been employed as a statistician on the scientific staff of the British Medical Research Council's pneumoconiosis unit in Penarth, Wales?

A. Yes.

Q. During that time a major portion of your work has been in research studies on the health effects of asbestos and I understand you have either authored or coauthored some twenty-five papers on the subject, both from an experimental point of view and an epidemiological point of view?

A. Yes.

Q. You were also a member of the working group of the International Agency for Research on Cancer that produced monograph number fourteen on the Evaluation of the Carcinogenic Risk of Asbestos to Man?

A. Yes.

Q. Finally, Mr. Berry, we have asked and you have indicated to us that you are speaking here entirely in a personal capacity and not in any official capacity on behalf of the British Medical Research Council.

A. That's correct, yes.

Q. Thank you very much for coming. The table is yours and the slide projector is yours.

MR. BERRY: A. Mr. Chairman, Commissioners, ladies and gentlemen...

MR. LASKIN: Feel free, Mr. Berry, by the way, if it's more comfortable, feel free to sit down. We are relatively informal. It's up to you.

MR. BERRY: Yes. Perhaps I'll do that then. Thank you.

I think there is now no serious doubt that exposure of man to asbestos has serious health implications, and I shall assume that this Commission does not require proof of this point, although in fact it will emerge in relation to other points, later.

I think it's also recognized that the health effects do not occur until several years after exposure - a minimum of ten years, and more typically twenty or more years.

This means that the increased cancer rates which we are now observing in asbestos and former asbestos workers is the result of conditions in the industry dating back many years. Most research is of workers who were exposed in the 1950's or the 1940's, or even earlier than that.

I don't know...well, I don't really know anything about conditions in Ontario over that time, but in the United Kingdom there have been big changes in the occupational environment.

Could I have the first slide, please?

Right. I'm just going to illustrate the point I just made about changes in the occupational environment with a couple of examples from my own work. This one relates to conditions in an asbestos textile factor. It's actually from the paper number six on your list...numbers five and six.

It shows the dust levels that occurred in this asbestos textile factory from 1935 to 1970, and you can see a big reduction over that period of time.

MR. BERRY: (cont'd.) Could I have the next slide, please?

MR. LASKIN: Can I just interrupt to say that that first slide, for the record, is at tab seven, page twenty-six.

MR. BERRY: That's it, yes.

This is from paper number fifteen on your list, and it shows a concentration of asbestos in a factory which was producing friction materials...brake lining and clutch linins... in the United Kingdom, from chrysotile asbestos. There are four periods of time given and there's four types of job.

You can see within every...with every type of work the reduction in dust levels. The significant dates there are 1931 - there were some asbestos regulations, and you can see the effect of those in reducing dust levels.

There were again some regulations in 1969, which introduced a two fiber per millilitre standard, and you can see the effect of that.

But there is also a reduction going on in the intervening time when there was no additional legislation but there was a continual improvement of working conditions.

Now because of this...we are going to have this slide up now, please...because of the latency which I've just spoken about, and the change in dust levels, we've got the problem of deciding the effects of current dust levels. As far as asbestos workers who have been exposed in the past are concerned, we have unfortunately got the problem that a lot of those are going to get disease, irrespective of anything, any legislation which might be passed, any improvements in conditions. A lot of disease is already determined.

This is illustrated by a paper which Dr. Newhouse and myself published in 1976. It isn't on your record, but Mr. Laskin has a reference. It's reference number twenty-one.

Where we attempted to predict the number of

MR. BERRY: (cont'd.) mesotheliomas which would occur in an exposed group who had worked in a factory in the east end of London, up to the year 2000.

Now, we had to make a number of assumptions, obviously, which only time will show whether they were correct. But our predictions were that the peak incidence of mesotheliomas would be in the early 1980's, and we were doing this work over five years ago. The factory closed in 1968, but we estimate the peak incidence of mesotheliomas will be in the 1980's. But even during the last five years of the twentieth century the incidence will be as high as it had been thirty years previously, so it takes a long time for the health effects to work their way through the exposed population.

There is no way of preventing this, and as far as I am aware there is little to suggest that there is going to be any imminent breakthrough to prevent disease in people already exposed.

The combined effects of reduced exposure and long latency means to some extent epidemiological research is historical research, rather than scientific research. But if we are to make it scientific research, we must include in it some attempt to guide our present and future policy.

What we are now interested in is the adverse health effects of present dust levels, and of levels which might be legislated either now or in years to come.

We won't know for certain what these effects are until twenty or thirty or forty years from now, but we must try and estimate what they are likely to be, and take action accordingly.

We can only answer these questions by extrapolation from previous high levels. Extrapolation is by no means a certain process. In fact, it's far from certain, and it's one of the reasons why scientists often seem reluctant to answer

MR. BERRY: (cont'd.) what are the most relevant questions and give undisputed guidance to legislative bodies and to inquiries such as this Commission.

Now, how can we use present knowledge to prevent or reduce health effects? Well, as asbestos is dangerous to health, a large exposure is more hazardous than a small exposure. That is to say, there is a dose-response relationship.

This is really a scientific principle that if something is harmful, then a lot of it is more harmful than a little, but it has been disputed on asbestos over the years, particularly for mesothelioma.

For lung cancer, the dose-response relationship has been demonstrated by Enterline and you heard him last week, so I've no doubt he showed you his data...and also by McDonald, who you are going to hear next week, so I'll not waste time showing their results, but they do demonstrate the point.

What I will do is show you some results from the factory in the east end of London which Dr. Newhouse and I have been studying for the last fifteen years or so.

Next slide, please.

Now, this...the results I'm going to show you are from a factory where asbestos products were made and all types of asbestos were used - that is, crocidolite, amosite and chrysotile were all used. There were both men and women employed in this factory, and the data start from 1933, following the regulations in 1931.

I'm sorry my slides are the wrong thickness and causing all this trouble.

We want to go on to the next one.

That's it. I think there was perhaps one other, but it doesn't matter.

In this factory we don't actually have any dust measurements, but what was done was to classify jobs as

MR. BERRY: (cont'd.) the light, moderate exposure or severe exposure, and we also divided the people according to how long they had worked - two years or less, or more than two years.

This is for women who would have severe exposure, but for a short period of time.

Now the next few slides are all going to be of this form, and what we have - we've got Observed and Expected Deaths. The top row we've got, All Causes. We then have Lung Cancer, then Pleural Mesothelioma, then we have Gastro-intestinal Cancer and Peritoneal Mesothelioma, Other Cancers, and Respiratory Disease and Asbestosis and Other Causes.

Of course, mesotheliomas are a very rare cause of death in the general population, so I haven't filled anything for expectation, but really the expectation is zero, as near as makes no difference.

In this one for women, severe exposure but for a short time, you can see that there were mesotheliomas, you can see that there was lung cancer in excess, and asterisks indicate statistical significance - that is, an excess which is unlikely to be due to chance.

Mesotheliomas occurred, there was an increase in gastrointestinal cancer which was not significant, and that is about all.

Can I have the next slide, please?

DR. UFFEN: Could I ask you a question for clarification before it's gone? With the lung cancer you've got two stars on it, where the observed to expected is just about four. But for gastrointestinal cancer there are no stars and the observed ratio is about two.

MR. BERRY: Yes.

DR. UFFEN: Is there somewhere between two and four that you put the stars on, or not?

MR. BERRY: Well, it isn't quite as simple as that. It depends not only on the ratio, but the numbers. For example, all causes is significant, although there it's only about one point three, but that's because we've got a lot of deaths.

DR. UFFEN: Very well. Can I also ask the question...can you keep the slide on...I believe this data that you are showing us is probably in item eight, table six?

MR. LASKIN: It's actually..I believe it's tab six, table six at page fifty-eight.

DR. UFFEN: Annals New York Academy of Sciences...

MR. BERRY: Yes.

DR. UFFEN: ...Health Hazard of Asbestos Exposure, the page that I'm looking at. Is that six?

MR. LASKIN: Yes.

MR. BERRY: Yes. There's five times the mortality in asbestos factory workers in London.

DR. UFFEN: On that, for gastrointestinal cancer, you show for your observed versus expected, for females with less than two years exposure, fourteen observed deaths versus five point seven expected. I presume that you have lumped together the peritoneal mesothelioma and the gastro-intestinal cancer? I don't understand the difference between the data in that table...if it's the same group.

That slide is not the right slide.

MR. BERRY: Yes, we've actually gone on to the other one.

MR. LASKIN: We can get the slide back.

DR. UFFEN: That's the correct one.

MR. BERRY: That's it, yes.

Yes, it is the same data as given on table six of the reference you are looking at, but it's set out slightly differently in that the pleural mesotheliomas and the peritoneum

MR. BERRY: (cont'd.) mesotheliomas are shown separately. Whereas in the table, they are shown in brackets and they are also included in the figure to the left of the brackets, so you've got there fourteen gastrointestinal cancers, here we've got ten, plus five peritoneal mesotheliomas...well, those five peritoneal mesotheliomas, four of them on the death certificate were put as gastrointestinal tumors, and one of them as something else. The figures in the table you are looking at refer to what was on the death certificate.

DR. UFFEN: Thank you.

MR. BERRY: We did some more careful inquiries and had the histology looked at to check for mesotheliomas.

MR. LASKIN: Just so that I'm...I'm sorry.

DR. MUSTARD: Can I ask the following question? Then I'm always troubled, as I'm sure you are in a statistical sense, about what your control estimate is.

MR. BERRY: Yes.

DR. MUSTARD: Is there a probability that in that control estimate on the righthand side, what is called gastro-intestinal cancer could possibly include some peritoneal mesotheliomas? I realize that's uncommon in nonasbestos-exposed workers, but there is always the possibility. Have we got an uncertainty on the righthand side as well?

MR. BERRY: Indeed, yes, and that is one reason why we show it that way in those tables, because then we are comparing what was on the death certificate with the expected values which are worked out from death certificate data on the whole population.

MR. LASKIN: Mr. Berry, just so that I'm clear before we go on, where you show peritoneal mesotheliomas - five, is then after death certificates are corrected, is the correct figure for gastrointestinal cancer then five, rather than ten?

MR. BERRY: No, it will be...the corrected figure...

MR. BERRY: (cont'd.) the original figure would be fourteen.

MR. LASKIN: The original figure was fourteen?

MR. BERRY: After one correction it would be fifteen, because one was a peritoneal mesothelioma and was recorded as something totally different, and then after correcting one stage further and taking out peritoneal mesotheliomas, then other GI cancers are ten.

MR. LASKIN: The second question, do I take it that even though there is no star beside pleural or peritoneal mesothelioma that those are statistically significant figures?

MR. BERRY: Yes. Yes, all mesotheliomas we would take as significant, becauseit's such a rare cause of death.

MR. LASKIN: Your confidence interval is...is it ninety-nine or ninety-five percent?

MR. BERRY: Two stars is significant at the one percent level.

Right. Shall we go on to the next slide?

This is severe exposure again, but for a longer period of time, and we've now got bigger excesses, three stars is significant of the point one percent level. You've got lung cancer in women raised twentyfold. We've got mesotheliomas again, even taking out the peritoneal mesotheliomas we've got an excess of gastrointestinal cancer and one star is significant at the five percent level.

We've got other cancer as in excess as well.

MR. LASKIN: Are those other cancers...because we haven't really had a female cohort before that we've looked at...are those other cancers cancers of the ovary and the breast?

MR. BERRY: I don't recall offhand. There would certainly be some cancers of the breast because that is quite a common condition in women, and I think there were some cancers

MR. BERRY: (cont'd.) of the ovary as well. I would just have to check the figures to tell you how many.

Right, can we go onto the next slide, please?

We are now moving on to men. This is light/moderate exposure for a short period of time. There is nothing statistically significant, although lung cancers are up a little bit, and there were mesotheliomas which are, of course, significant. But apart from the mesotheliomas and a suggestion of a small increase in lung cancer, there is nothing remarkable showing there.

That is for the lightest exposure to the shortest period of time.

Could I have the next slide, please?

This is the light/moderate exposure, but for a longer period of time.

Could you just move that over to the left a little?

Again, we've got mesotheliomas, we've got lung cancers raised, but not significantly so.

The next one, please.

We are now moving on to severe exposure for a short time, and you can now see, although it was for a short time that there are significant effects - a lot of mesotheliomas, there is a doubling of the lung cancer rate, there is an increase in gastrointestinal cancers, although not significant, and other cancers are significantly raised.

MR. LASKIN: Could you just tell us what your guidelines are for distinguishing between light/moderate and severe exposure?

MR. BERRY: Yes. This was...as I said, there were no dust measurements. These were based on talking to people who have been in the factory for a long time, on what the dust conditions had been. So in a sense it is quite a crude distinction. There is a possibility of errors in particular jobs.

MR. BERRY: (cont'd.) The fact that we are getting this dose-response relationship which we are seeing, shows that on average it was a good distinction, but there is a possibility that particular jobs got into the wrong category, and this would make our comparisons less sensitive.

But the dose-response relationship shows up very well, so that what was done is shown to have been a pretty good way of assessing the conditions, in a qualitative sense. But of course it doesn't help us in a quantitative sense because we've got no dust measures...or very little, very few, to assign to these conditions.

DR. DUPRE: Mr. Berry, if I may, there are six pleural mesotheliomas here observed among nine hundred and forty-two severely exposed men. If I remember the previous slide correctly, there were four pleural mesotheliomas observed among five hundred lightly exposed men. Is my memory correct of the last slide? Is it six and four?

MR. BERRY: Yes, there would be...yes, there were seven mesotheliomas. Most of them were peritoneal though, but yes, there were seven mesotheliomas.

But I think there was only one pleural, we'll see in a minute.

DR. DUPRE: I may not be recalling the previous slide correctly. That's why I'm asking.

Yes, four pleural mesotheliomas among five hundred and sixty-nine...

MR. BERRY: Oh...

DR. DUPRE: ...lightly exposed, and then six among nine hundred-some severely exposed.

MR. BERRY: Yes. Yes, this isn't quite the same as the table. It must be...yes, I thought I was showing slides which were exactly the same as this table, but this must be a slightly different, a slightly later analysis of the data.

MR. BERRY: (cont'd.) So there are now, yes, eight mesotheliomas. Yes, that is correct.

MR. LASKIN: I take it it's similar to the information in table one at page 54, but it's maybe a slightly updated version?

MR. BERRY: Yes. Yes, I'm sorry it isn't identical. As I say, I thought I was showing identical data, but it must be an analysis that's a slightly different time. There are more deaths here, then it looks as if it's at a later time.

MR. LASKIN: Perhaps I can, Mr. Chairman, assure everybody I will arrange one way or another to have these slides reproduced. We will either type them or have them reproduced, and then circulated to everyone.

MR. BERRY: Yes, although there are slight differences like this, I think I would prefer to stick to what is in print.

But I think we'll find the differences are quite, quite minor, in general.

All right, shall I go on, Mr. Chairman?

DR. DUPRE: If you please, sir.

MR. BERRY: Right. If we move on now, we'll have to move on two slides.

The next one, please, yes.

We've now got severe exposure for more than two years, and you can see even bigger effects. Lung cancer is now five times expectation, other cancers are significant, gastrointestinal cancer is raised, but not significantly, and there are eighteen mesotheliomas.

Again, this is slightly different to what is in the actual paper, but the differences are quite minor.

Right, could I have the next slide, please?

This is a summary of the data for men, with

MR. BERRY: (cont'd.) the four types of exposure, two severities and two lengths, and this is the lung cancer... excluding the mesothelioma...it gives observed, expected and the ratio of the two.

The ratio is multiplied by a hundred, so if it's bigger than a hundred that indicates an increase. You can see the dose-response relationship. Both severe exposure gives more cases, a bigger excess than light/moderate, and the length of exposure increases the excess as well.

Could I have the next slide, please?

Now, this is for mesotheliomas where they have been worked out as the rate per hundred thousand man years of followup, and this is a more significant slide because, as I indicated, other people have shown a dose-response for lung cancer, but not many people have looked at it for mesothelioma. Of course, mesotheliomas are known to occur after incidental exposure which..

DR. MUSTARD: Excuse me. These are all mesotheliomas, lung and...?

MR. BERRY: Yes, correct, pleural and peritoneal.

Mesotheliomas are known to occur after incidental exposure, and this has led to several people putting forward the opinion that there was no dose-response relationship.

The problem with that argument was that the cases had been observed, but nobody knew how many people were at risk. In other words, we didn't know the denominator necessary to calculate an incidence rate.

But in this study where we know the number of cases and we know how many people were at risk for how long, we can work out the rate and we'll get the figures in the righthand column, and again there is a dose-response relationship. Less than two years gives fewer than more than two years, and light/moderate gives fewer than severe.

MR. BERRY: (cont'd.) I haven't got a slide of this, but there is a similar sort of result given in our paper on the gas mask workers, it's paper number eleven on your list, where sixteen mesotheliomas had been observed in a group of women who were employed making gas masks with crocidolite asbestos during the last war, and out of three hundred women with less than five months exposure, there had been no mesotheliomas. But in women with more than five months exposure, there had been sixteen out of four hundred.

So you've got there two groups of approximately the same size. One gave zero mesotheliomas, and the other gave sixteen. The difference between those two groups was that one had had short exposure and the other had had a longer exposure, but still not a long one, only an average of about two years.

MR. LASKIN: Mr. Berry, the figures that are on the slide, are they a result of just looking at the death certificate, or are they as a result of somebody going behind the death certificates?

MR. BERRY: They are a result of the latter. What we did was, Dr. Newhouse wrote to the hospitals where people had died to find out if there had been a post mortem, and if they had, she obtained the material, which was submitted to Dr. Bogner, who is a pathologist working at the unit where I work, and he looked at these to see if there was any sign of a mesothelioma.

MR. LASKIN: Would Dr. Newhouse have post mortems on most of the deaths?

MR. BERRY: I think it was around about fifty percent. It's in one of the papers.

There were certain things on the death certificates which gave a clue as to whether it might have been a mesothelioma, and of course she was concentrating on those.

MR. LASKIN: Does that suggest then that to

MR. LASKIN: (cont'd.) the extent that she hadn't looked at the other...there weren't post mortems for the other fifty percent that even these figures may have underestimated the risk of mesothelioma?

MR. BERRY: Certainly, yes. If there was a mesothelioma in someone without a post mortem, and it wasn't mentioned on the death certificate, then of course there will be no way that we could pick that up later.

One would expect that there would have been a few.

MR. LASKIN: Were there many death certificates where in fact mesothelioma was stated to be the cause of death?

MR. BERRY: Not in the early years, no, because it was only relatively recently that mesothelioma has been... has had a code of its own in the international classification of death. But now, of course, the situation is different. But most of this is data which occurred before it would be coded on the death certificate.

MR. LASKIN: Do you, offhand, know when that date was?

MR. BERRY: When the ICD code changed. I think it maybe came in in 1968 with the eighth revision, but I'm not sure.

MR. LASKIN: Prior to that time there was no separate classification for mesiothelioma as a cause of death?

MR. BERRY: That's right, yes.

DR. DUPRE: Mr. Berry, could I just try to put this in the context of the other two slides that I questioned you about? To my layman's naked eye, the two slides on which I commented earlier did not seem to show anything very much in the way of a dose-response relationship where mesotheliomas are concered.

MR. BERRY: Yes.

DR. DUPRE: And again, and I'm speaking strictly of a layman's naked eye, this slide, of course, does seem perhaps to suggest a dose-response relationship?

MR. BERRY: Yes. I think, Mr. Chairman, you were concentrating on the pleural mesotheliomas, whereas this includes...

DR. DUPRE: This is all?

MR. BERRY: Both, yes.

DR. DUPRE: You are not able to distinguish them?

MR. BERRY: Well, we didn't...we weren't able to detect any pattern of different, say, dose-response relationships. It looked a little bit random, so we combined them.

DR. DUPRE: I see. So in terms of what you are saying, there really is not any more evidence of a dose-response relationship where pleural mesotheliomas are concerned, on this slide, than there was on the other two slides?

MR. BERRY: No. I think also, Mr. Chairman, the contrast that you queried at the time was between the light/moderate greater than two years, and the severe less than two years, which you will see give ...

DR. DUPRE: I see.

MR. BERRY: ...identical figures here.

The length of exposure is less in the severe, so that's indicating that the severity is putting the rate up, which again you can see contrasting the severe less than two years with the light/moderate less than two years, you've got a threefold increase, and if you contrast them for greater than two years you've got a two-and-a-halffold increase.

Right, could we have the slide up for a moment now, please?

Given that there is a dose-response relationship for asbestos-related diseases, then it follows that we can reduce those diseases by reducing dust levels. Some people

MR. BERRY: (cont'd.) have argued that a solution is to ban asbestos, and that adequate substitutes are now available.

Well, I'm not competent to discuss whether in fact there are adequate substitutes for jobs such as brakes on automobiles, or for effective fire prevention. I think that much of insualtion that was formerly done by asbestos is now done by substitute material.

So as long as there aren't substitutes for all uses of asbestos, then we will presumably be continuing to use it, and if that's the case then we have to control the exposure by some means or other.

The ways that have been used in the past have been variously called threshold limit values, hygience standards, and the new name for these in the United Kingdom is control limits. But in fact these are all different names for the same thing...or more or less the same thing.

An important document in this field was, of course, the 1968 report of the British Occupational Hygiene Society, from which the United Kingdom government extracted a two fiber per ML limit.

This report has received a lot of criticism over the years, and indeed there were many inadequacies which were worthy of criticism. When it proved possible, we did a better study, which was published not as a standard-setting exercise, but as a paper, and it's paper number five.

This later study modified some of the previous conclusions, and it also showed a number of difficulties which hadn't previously been realized.

MR. LASKIN: Mr. Berry, could you...

DR. DUPRE: Number five, Information Obtained from Animal Experiments?

MR. BERRY: No, no.

DR. DUPRE: I think we've got it incorrectly.

MR. LASKIN: Could you just read the article out, because I think our co-ordinated numbering system has broken down here?

MR. BERRY: Oh, I see. Yes. The one I'm referring to is Asbestosis - A Study of Dose-Response Relationships in an Asbestos Textile Factory, British Journal of Industrial Medicine.

MR. LASKIN: I think what might assist, if I give Mr. Berry the updated index.

MR. BERRY: Thank you. Yes, it's number eight, Mr. Chairman, on this revised list.

Now, could I have the slide, please?

Well, you've seen this slide before when I talked about the problem of extrapolating from high values to low values. The situation is by no means as simple as I, at that point, indicated.

If you look at this slide in detail, you will see that the lines are given in different types of symbols. Some of them refer to estimated dust levels up to 1950. We then have levels measured with a thermal precipitator, that is to say, the count was in particles instead of in fibers, and we then have measured fiber counts.

So that it isn't just a matter of extrapolating from high dust levels. We have the problem that we don't have a long series of dust data all collected by the same method. Apart from what is shown on this slide, we have other complications. We have the complication of going from static to personal samplers, and we have the method of assessing samples which is now done by graticule counting, which came in a few years ago almost without anybody noticing, so that in the United Kingdom the hygiene standard was in effect halved by no statute, no legislation whatsoever - simply by a difference in the way that

MR. BERRY: (cont'd.) slides were counted.

MR. LASKIN: Could you elaborate on that a little bit for us?

MR. BERRY: Not a great deal, because it's outside my field. But I've no doubt someone like Dr. Gibbs can tell you a lot more, when you hear him.

But it's to do with the way that people view microscope slides. Formerly they just looked at a field, and now they put a graticule on it to guide them in their counting.

MR. LASKIN: But you are suggesting that a practical result of that changeover was really to halve the existing standards?

MR. BERRY: I'm not actually suggesting that myself. I'm, in effect, reading from the Simpson report where they looked at this, and that was their conclusion. I've done no direct work myself on that point.

Now, these conversions from one method to another are very difficult. They introduce uncertainties, and quite a lot of people would say, in fact, that it's impossible to do it. I could only wish that I was confident that the lessons of the past have been learned by those who measure environment, and that they won't continue to change their methods without carefully calibrating the new method with the old method before they introduce the new, because if they do, we'll be in the same situation twenty years from now.

DR. UFFEN: Before you take that slide off, could I just...when I found it in the particular paper here, I noticed the docket part, the early part of the curve, was estimated, and in the text you said it may have been underestimated? Am I correct?

MR. BERRY: Yes. The early period was estimated is probably not a very good word...guesstimated would be a

MR. BERRY: (cont'd.) better word, which is a slang word which people use...and it was based on knowledge of how the ventilation had changed in the factory, and things like that. But there were no measurements at all, so it's an intelligent guess by people who are familiar with the process.

DR. UFFEN: For those of us that aren't familiar, is there any way you can give us an indication of the spread that might exist? In other words, if I put question marks on those first three points as being guesstimates, could it be double that, or ten times that, or half? Is there some measure of the range?

MR. BERRY: Yes. I don't think I can, in any helpful sense. No. I mean I don't think it could be half that, because that would mean the dust conditions were lower than was measured later. I doubt if it was as far out as tenfold, but I wouldn't...I couldn't really put an upper limit on it and I doubt if people with, you know more about it than me. I don't think they would want to be too specific either.

But, in the paper that we are quoting from, we got around this by doing the main analysis on the post-1951 group, so as not to allow any uncertainty in those figures to influence the results.

DR. UFFEN: In some of your papers you talk about the importance of the time?

MR. BERRY: Yes.

DR. UFFEN: I find...I hope that it will emerge today...to what degree these two are dependent, the time aspect that you made a study of, and the unreliability of the original data, because I believe you say it's the long time ago that is by far the most important?

MR. BERRY: That is so, yes. But in the analysis, I still restrict it to people employed after 1950, so that the

MR. BERRY: (cont'd.) time ago in the past only goes twenty-five years. I don't use this early data. Not in that part of the analysis.

If we had...if we could replace reliance on these data, then we would have been able to do a much better analysis, but we didn't feel that we could place reliance on these half-guessed figures.

MR. LASKIN: For the record, that's figure one, tab seven, page 186.

MR. BERRY: This factory was, in fact, in the forefront of environmental measurement in asbestos-exposed populations. If this is in the forefront, you can imagine that things are a lot worse in other places. But as we have already indicated, at the factory that we were studying in the east end of London, we didn't have dust levels at all.

Now, a second difficulty is that it is thought prudent to set standards which offer a certain degree of protection over a lifetime's exposure. That is, say, forty or fifty years. But we usually are working with data over a shorter period of time. In this case, twenty-five years, because we are not prepared to use the early dust data. So we are extrapolating over time, as well as over dust level.

Now, this is a point which I go into in quite a lot of detail in reference number eight, and I'll show one or two slides on this aspect.

Now, the problem is, we have people exposed for many years of time, to changing concentrations and at the end of the day they are observed with some asbestos-related disease.

The question is, how do we combine all this exposure to produce a measure which we can put on a dose-response graph?

The traditional way has been what is called cumulative exposure. That is simply the average concentration

MR. BERRY: (cont'd.) in each year just summed over the whole period of exposure.

So if a man is exposed to ten fibers per millilitre for a year, that counts as ten towards a cumulative exposure. It doesn't matter when that year was, it doesn't matter whether it was last year or twenty years ago.

Now, we thought this was a little unrealistic. We know we've got a latent period...that is, we know that exposure a long time ago is more likely to be the cause of the disease than exposure last year. We know that progression of disease takes place, even after exposure has stopped.

Could I have the next slide, please?

The next measure we looked at was one that had been put forward by Jahr, where he weighted each contribution to the exposure by the time that had elapsed since that exposure took place. So that he would go back to somebody exposed to ten fibers per millilitre for one year, he will give that a weight of one if it was last year, but a weight of ten if it was ten years ago, or twenty if it was twenty years ago. So that will contribute to this measure ten units, if it was last year, and two hundred if it was twenty years ago.

So the exposure long time ago is being considered a lot more important.

Could I have the next slide, please?

Now, this measure is one which we brought in which is an elaboration of that one given by Jahr, where instead of weighting by the time that has elapsed since the dust was inhaled, we weighted by the time that the dust remained in the lung, assuming that there was elimination going on.

We then summed that over the whole period of exposure.

Now, it can be shown by a little bit of

MR. BERRY: (cont'd.) mathematics that the ... if one accepts this concept, this slide, then cumulative exposure, the first one, in effect assumes that dust is eliminated almost instantaneously. It is inhaled and it causes some biological reaction, but it is then either eliminated or inactivated.

Jahr's measure, which we saw in the last slide, assumes that there is no elimination. That is, the elimination rate is zero.

So those two measures, cumulative exposure and Jahr's measure, are at the extreme ends, on opposite ends, of the whole family of exposure measures which is indicated by this slide, and the family of measures is indexed by how fast the elimination is.

DR. MUSTARD: Excuse me. Is there evidence of elimination, or is that just an assumption?

MR. BERRY: There is evidence from animal experiments, yes.

DR. MUSTARD: Of complete elimination?

MR. BERRY: No, no. Not of complete elimination.

DR. MUSTARD: But the assumption is for complete elimination?

MR. BERRY: No, the assumption is elimination by an exponential process.

DR. MUSTARD: Based on the animal experiments?

MR. BERRY: Well, not really. There isn't sufficient animal data, I don't think, to show that it is exponential.

DR. UFFEN: You did try to get a decay constant from animal experiments?

MR. BERRY: No, not really, no.

DR. UFFEN: Then you get the idea that you said that the evidence for determining decay constant or the half life is very unreliable?

MR. BERRY: Correct.

DR. UFFEN: Where did it come from, do you know?

MR. BERRY: It came from these data from this study.

DR. UFFEN: Not from animals?

MR. BERRY: No.

Now, the problem is, as the Commissioner just indicated, that we don't know the decay constant...that is the rate of elimination. And what we did was, we included it as a parameter to be estimated from the data, but it proved impossible to estimate it with any sort of precision that would be useful. Almost a whole range was acceptable as a summary of the data. Not quite the whole range, we were able to exclude cumulative exposure, but the half-life time of elimination could have been as short as three years. That would not have been contradicted by the data, and it could have been a zero elimination rate - that is Yaw's measure - and that was not contradicted by the data. So this is a huge range.

I wonder if I could have the next slide, please?

The one we are going to see is from reference number six, and it is given as a figure in that reference.

This indicates a hypothetical group of men exposed to asbestos at five fibers per millilitre for twenty years, and exposure is indicated by that hatching at the bottom.

They are followed through that exposure, and then for twenty years thereafter.

MR. LASKIN: I think it's tab seven, I understand, Mr. Berry, at page 190.

MR. BERRY: Oh, I'm sorry. My mistake, yes. I had two papers there, unfortunately. Yes.

Tab seven.

We are looking at the prevalence of possible asbestosis, and there are four different, five different graphs

MR. BERRY: (cont'd.) to show. We've got at the bottom, that based on assuming that cumulative dose is the correct way to measure exposure. We've got the one at the top assuming that there was no elimination whatsoever, and then we've got three in between assuming that elimination took place with half-life times of five, ten and twenty-five years.

You can see that up to twenty years there was no difference between the predicted disease prevalence. This wasn't a coincidence. It was because twenty years exposure to five fibers was the average for the data, and the estimates used have been fitted to those data so they are bound to be at the same point after twenty years.

But thereafter, there are quite big differences. If we assume cumulative dose...which as I say, we can't actually reject that one...but that would say there is no further disease because there was no further exposure.

If we assume no elimination, we could go up from about seven percent to over thirty percent disease over the next twenty years.

The other ones are obviously in between.

Could I have the next one, please?

This is from the same paper or this might be an extract from number eight. It's either from number eight or from number seven. They were both on the same data.

This is the same sort of thing where here we are looking at thirty, forty or fifty years exposure to two fibers per millilitre and seeing what the disease prevalence would be at the end of such a period of exposure.

If we just look down, say, the forty year column, we can see it varies from four to fourteen percent. That's obviously a big range.

Now, I said that the papers we wrote in 1979 on this study, which was a further study of the same population

MR. BERRY: (cont'd.) which had been looked at by the British Occupational Hygiene Society in 1968. It wasn't a standard-setting paper, but a lot of people have read it as though it was, and I can only accept responsibility that that is due to the writer's not making it clear.

But I would like to emphasize that there are various figures given in that paper. We are not advocating that any of them should be standards, or control limits, whatever you care to call them. We are really trying to put forward the difficulty of choosing what to have as standards because of the vast uncertainty which this extrapolation process, which I have mentioned, introduces.

One of the purposes of this paper was to show this uncertainty and to move away from making what might appear to be more certain predictions.

MR. LASKIN: Again, for the record, that we are looking at table two, tab seven, page 191.

MR. BERRY: Now, in the paper number eight where we did make the point that we weren't too sure of some of the aspects of the response variable we used, which was possible asbestosis, and we have been criticized for using this measure. It was a clinical type judgement which was made based on x-rays, lung function and whether crepitations were present or not, and we weren't sure whether it indicated disease or not.

Well, we've now got a bit more data, and if I can go on to the next slide?

Yes, within that paper...I'm pretty sure this will be from number eight...we put this graph in. We did show that people who were recorded as having possibly asbestosis, a lot of them, sixty percent of them, ended up as having certified asbestosis...I'll explain in a minute what that means...within five years. So that a lot of them go on to some further stage of disease within a fairly short time.

MR. BERRY: (cont'd.) Now what I mean by certified asbestosis is, asbestosis which has been accepted for compensation purposes by the British pneumoconiosis medical panels.

THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

Edwina Macht
EDWINA MACHT

.....page 32

THE WITNESS: (cont'd.) Now, since we published this paper in 1979, I've done another study, which has just appeared in the British Journal of Industrial Medicine; it's number 16 on this index, but I notice there's a mistake there.

It wasn't given at the British Occupational Hygiene Society meeting in Cardiff in 1980; it was actually published in the British Journal of Industrial Medicine, and I saw a copy of it about a fortnight ago. I don't know whether it's reached this side of the Atlantic yet or not, but I think you have pre-publication draft, which Mr. Laskin distributed.

MR. LASKIN: Mr. Berry, just before you go on, let's identify that slide for the record also; it's at tab 8, page 103, figure 2.

THE WITNESS: Right. Now, in this study which I'm going to describe now, what we did was to look at people who'd been certified by pneumoconiosis medical panels and look at their subsequent mortality, because we've established here a link between possible asbestosis and certified asbestosis, but we don't know what that means in terms of mortality.

So could I have the next slide, please.

What we -- while that's going on, I'll give you the background to this. We took people (men) from two of the pneumoconiosis medical panels; we extracted their records, found out whether they'd died or not and what the cause of death was.

And the pneumoconiosis medical panels, they -- if they certify somebody, they give a percentage award, which is in units of ten per cent, so if there's little disability they give ten per cent or twenty per cent, and they give higher rates if there's more disability. And this slide divided the men according to the percentage disability.

But the key points I'd like you to observe are that, even with ten or twenty per cent disability (that is, the

THE WITNESS: (cont'd.) lowest rate that they could compensate them at), there was increased lung cancer, there was one mesothelioma, and there were deaths due to asbestosis.

And as you go across the table, you get increasing lung cancer excesses, in relative terms, with increasing disability percentage; fifty per cent, there is a sixteenfold increase, compared with a threefold increase at ten or twenty per cent.

MR. IASKIN: Can I just identify this for the record, so everybody knows what it is. I think we are at table 7 of tab 16 -- I'll show it to Mr. Berry -- and I think we're on the lower chart, and I think that slide simply adds together the first two columns, which are the ten and twenty per cent disability benefits.

MR. BERRY: That is correct; yes. Yes; it is shown in a bit more detail in the paper.

DR. MUSTARD: I wonder if you could amplify a little bit the middle column for me in that table. You have forty-seven individuals classified as having thirty to forty per cent disability, of which there were thirty-one deaths versus eight point five expected.

The deaths you have listed as occurring are fourteen from lung cancer, three from mesothelioma, and five for asbestosis which, if I add those up to the fourteen, comes to twenty-two, leaving me a gap of nine deaths from other causes not shown on there.

I wonder if -- what those other columns are; whether they are all unrelated to asbestos exposure or possibly might also be asbestos exposure.

MR. BERRY: Yes. We looked, in particular, at gastrointestinal cancer and found no excess.

MR. BERRY: (cont'd.) There is another table, I think, that shows that. Yes; here's table 5, although that doesn't separate out gastrointestinal cancers; it's actually discussed in the text. But we certainly looked at gastrointestinal cancer because we were expecting that that might be raised, but, in fact, it wasn't.

DR. MUSTARD: But this is a cohort selected out because they'd already been given compensation for asbestosis?

MR. BERRY: Correct; yes.

DR. DUPRE: Mr. Berry, I'm just looking at table 5 in that same article, simply because I'm interested on the breakdown on the lefthand side. On some of the other tables not associated with asbestos disease and no informations are lumped together.

But here, on table 5, not associated with asbestos disease shows up separately. Does your paper indicate what the causes of death were, when they were categorized as not associated with asbestos disease?

MR. BERRY: I don't think we give that in detail, no. A lot of them, I expect, would be due -- oh, no; we've got coronary disease separate. No, it doesn't indicate.

DR. UFFEN: You may have already said this and I missed it. Are these causes taken from death certificates or from a post mortem?

MR. BERRY: With -- the usual practice is that, if somebody is known about by a pneumoconiosis medical panel, then there will be a post mortem, so it'd be post mortems in over ninety per cent of these people.

DR. UFFEN: Of the ones that died of other causes, that would be just from a death certificate, would it?

MR. BERRY: Oh, no; there would still be a post mortem.

DR. UFFEN: About ninety per cent?

MR. BERRY: Yes.

DR. UFFEN: And one other thing, just for my clarification. We are equating disability with compensation. When we say a person had a thirty percent disability, what we're really saying is some medical assessment has been made and a judgment has been made, and he had been assigned a compensation?

MR. BERRY: Correct.

DR. UFFEN: So there's a medical judgment between the disability and compensation?

MR. BERRY: Yes.

Could I have the next slide, please. Well, this is a similar slide shown for the London panel, and it's based on table -- the top half of table 6 of the reference; and it shows a similar sort of thing but, again, I would emphasize that lung cancer is increased, even for those with the lowest disability percentage.

And if you look at the original reference, you'll see, even for those -- it's not table 6, is it; it's table 7 -- even with those with ten per cent, taking those separately from the twenty per cent, there's an increase in lung cancer deaths, and there are also mesotheliomas; and these excess rates of death increase as you go across the compensation -- increasing compensation.

It might not be too obvious that that is the case from there, but if you look at the figures at the top, the number of men -- you see the number of men are declining quite rapidly because it's rare for somebody to be compensated at the fifty per cent level, because that would indicate that they've been a little slow going to the panel; so most people ---

DR. UFFEN: Or the panel have made a mistake?

MR. BERRY: No; it would -- well, I suppose that could be possible in some cases. See; people go to the panel by their own effort, and they might be advised by a factory doctor to go, or by their general physician.

The panel might turn them down, and they can then re-apply and, if they then were awarded fifty per cent, I suppose that would indicate the panel had made a mistake the first time. I don't know whether that happened in these data or not.

But if somebody came along for the first time and ended up in the fifty per cent category, that would indicate that, if he'd applied a few years earlier, he would have probably been compensated at the ten per cent level; and, in fact, that's where most of them start out: in the ten per cent or twenty per cent level.

Could I have the next slide, please. Let's go on to the next one. This just is a plot of observed mortality of these three different groups of compensation against what would be expected if these people had been dying at national rates. And you can see the type of -- you can see that there's -- people are dying sooner than they would otherwise have done, and that this is related to the degree of compensation, quite markedly.

Could we have the next slide, please.

DR. MUSTARD: Sorry; can I ask you a question. Years after certification is what that states. In view of our earlier discussion on elimination of fibres, were these people left in those conditions to which they could still be exposed to asbestos, or were they actually removed from exposure to asbestos?

MR. BERRY: Mmm -- I've got no direct evidence from these men; but, in general, the fact that they've been compensated does not mean that they're obliged to give up working

MR. BERRY: (cont'd.) with asbestos; that would be their decision.

In some cases, if they were working at a factory which had a range of working conditions, they would probably be moved to an area where there was less asbestos.

For example, if that had happened at the asbestos textile factory mentioned -- which we've been talking about a few minutes ago -- then they would certainly have been transferred to less dusty conditions; but they don't have to move. They can carry on doing the same work, if they wish to do so.

DR. MUSTARD: Can I ask you, would a person with a fifty per cent disability, however, be left working, under the British system?

MR. BERRY: Mmm -- I think if he -- I'll just give you my impression here; I don't know for certain -- but I think if he was as high as fifty per cent, then that would mean that he would have to have a job with reduced effort, so he would probably be obliged to change his job because his health was -- his respiratory health was so bad. But I emphasize, that's my impression, and I don't know whether those -- what happened to these men in this study and how they were working.

MR. LASKIN: Again for the record, that's figure 4 in tab 16.

MR. BERRY: And could I have the next slide, please. This is table 11 of that reference, which just gives the reduction in expectation of life. A lot of the figures have been shown, previously, particularly those of the study of the factory in London, were in terms of observed and expected mortality, and it's not always easy to see what the implications of that are.

So, in this case, we worked out how much life expectancy these people had lost; and even for ten per cent, we

MR. BERRY: (cont'd.) estimate a reduction of three years in life expectancy following their acceptance as asbestotics by the panel; and this rises to twelve years for those with fifty per cent or more, so these are big reductions in men who get as far as being accepted by the medical panels.

Of course, that means they are a selected group; they are, in fact, those people who have had the most ill effects.

DR. DUPRE: Now, these reductions are based on observed deaths among individuals at these different levels of disability?

MR. BERRY: Yes.

DR. DUPRE: And it is based on observed deaths from all causes?

MR. BERRY: Yes.

DR. DUPRE: So it is not based on -- if there is any shadow area where it's not entirely clear whether it's asbestos-related or not, that is not a factor in the figures on the righthand ---

MR. BERRY: That's correct; yes. Yes; and there are a number of problems in, you know -- in that -- because most of these men have post mortems, so it follows that their cause of death is more accurately determined than the national population; so, in that sense, the observed and expected aren't comparable.

But this is based on all deaths, and whether they did a post mortem or not, death is a pretty definite event, and it's usually accurately recorded as to when it took place. And so these figures don't depend on anything that happened at post mortem at all.

MR. LASKIN: Could you just -- if that's a way of looking at it, could you just tell me, how do you get to that

MR. LASKIN: (cont'd) figure and what are the ingredients that go into the reduction in years that you have?

MR. BERRY: Yes; well, the ingredients are the death rates from all causes, as compared with the national population's, the ratio of those death rates, so one can extract those from the tables in the paper and we then apply those to a life table, altering the national death rates by this factor and do the calculation -- it's a life table calculation, where the death rates are modified according to the excess that was observed.

Right. Well, if I can change -- well, before changing topic, I'll just go back again and say we did this study partly to see -- to complete the chain from our previous paper, possible asbestosis, certified asbestosis, and mortality. We've shown that most possible asbestosis people ended up in the certified category and, if you end up in the certified category, even if it's at the lowest level, you've then got a poor prognosis; and, therefore, we feel that our original use of possible asbestosis, which, in the paper, we were a little hesitant about and said we weren't sure exactly what it meant, I think we can now see that it does mean there is a definite disease which has serious implications.

If I can change topic now and move onto fibre type; now, for all practical purposes, there are three types of asbestos, because anthophyllite is not mined any more; we've got chrysotile, crocidolite, and amosite -- and crocidolite and amosite are usually referred to as the amphiboles.

The original -- we could have this slide off, please -- the original evidence of an association between asbestos and mesothelioma, which was a classic paper by Wagner, Sleggs, and Marshland, was between crocidolite asbestos and mesothelioma; and, since then, the association with crocidolite has

MR. BERRY: (cont'd.) been demonstrated several times. And there's also evidence, particularly from the United States, where they tended to use amosite during the war years when, in Britain, we were using crocidolite -- evidence from the United States has implicated amosite in the production of mesotheliomas.

Of great interest is the contrast between amphibole and chrysotile. Now, I've already spoken about -- shown some dust data from a friction plant, where chrysotile was used almost exclusively; but, in fact, crocidolite had been used on a particular contract. It was actually a contract for producing brake blocks for railways of some country in Africa.

And crocidolite was used for two periods of time of about four years' length in each case; from 1929 to 1933, and from about 1940 to 1944. And while it was being used, the people who were working on that contract -- it was being done at one end of the one of the sheds, so the crocidolite was in a confined place, and there were only a few people working on that contract.

Again, this is in conjunction with Dr. Newhouse; we've done a follow-up study of all the work force of this factory, and if I could show these slides, then we'll get along to the mesothelioma picture in a minute. This is just to show you how -- the size of the work force we're talking about, actually over thirteen thousand people, of whom almost two thousand have died. And you'll notice that we traced nearly all of them; over ninety-nine per cent were traced.

Next slide, please. This is all from tab 15; these slides will be extracts from the tables; the tables are given in more detail. This is for men, and if you look at the righthand part, we're looking at cancer of the lung and pleura, and we're comparing observed with expected; the same type as we've seen

MR. BERRY (cont'd.) before. And we've divided the men into -- according to the length of employment. The figures in brackets are pleural mesothelioma; and the main point about that is -- or the two main points -- one is that there were eight mesothelioma, but, apart from that, there was no excess of lung cancer, and there was no sign, even when we got to the five-year-plus group, of any excess. So, if we take out the eight mesotheliomas, we've got seventy-six lung cancers and we would have expected seventy-seven in this -- in a population of this size and age composition.

So could we have the next one, please. And this is the same sort of tabulation for women. It shows exactly the same sort of picture, and there were two mesotheliomas.

Could we have the next slide, please. In the previous slides, we included everybody, some people that had only worked for a short period, so we, here, concentrated on those with twenty years' exposure and with ten or more years of follow-up; so we're now looking at beyond thirty years after first exposure, so we've got the latency period well covered.

Again, you've got the mesotheliomas, but if you take out the mesotheliomas, the increase in lung cancer is there but it's not very big. Twenty-five lung cancers compared with nineteen expected and, in women, one compared with point nine expected. So, again, the main feature is these mesotheliomas; but, apart from that, not very much evidence of an increase in lung cancer.

MR. LASKIN: What is the statistically significant figure; is it the thirty-two?

MR. BERRY: That is the thirty-two.

MR. LASKIN: And if you took out the seven and made it twenty-five, would the twenty-five be statistically significant?

MR. BERRY: No, no; it is the seven which is making it significant.

Right; could we have the next slide, please. Now, what we did, to look at this in more detail, was looked at those ten mesotheliomas and chose some controls which were matched for certain variables, which are indicated there. The main ones are that they were matched for the year that they started in the factory, so that we're covering the same periods of latency; and the fifth one, that they were employed at the factory when crocidolite was being used, in the same way as the cases were.

And we did this as a case control study because it was a method put forward by McDonald and Thomas a few years ago -- in fact, just when we were setting up this study -- it enables one to extract from the records accurate detail on the cases of interest and of appropriately chosen controls, instead of doing that over the whole population and not being able to do it as carefully. So we were taking a lot of care over these fifty cases and controls.

Could I have the next slide, please. What we were looking at was to see whether these people had been exposed to crocidolite on this contract which I mentioned, and it turned out that eight of ten had worked on that contract, compared with only three out of forty of the controls; and the controls were working in the factory at the same time, but they were working in different sheds, or at the other end of the same shed, possibly.

There was a slight complication, if I could have the next slide, in that the mesotheliomas were also exposed to more chrysotile, because, if they'd been working on the railway contract, that means they were production workers who did a particular type of work; and when that contract ended and they went on to other work, there would be higher dust levels, which would

MR. BERRY: (cont'd.) be chrysotile.

So the mesotheliomas, more of them had been exposed to chrysotile at a moderate level of five fibres per millilitre. I say "moderate"; that is in historical terms. At this time, five fibres would be a moderate level, and only twenty-five per cent of the controls were so exposed.

So could I have a look at the next slide, please. So what we did to eliminate this confounding effect of chrysotile, we restricted the analysis to those mesotheliomas and their controls who'd all been exposed to chrysotile at five fibres per millilitre, and this meant that a lot of the controls were eliminated, because they'd been -- hadn't been exposed to that level; and we ended up with much less data: only six mesotheliomas and ten controls. But five of those mesotheliomas had been exposed to crocidolite, as indicated by the pluses in that table, compared with only two of their controls.

And the probability that as many as five of them had been so exposed, it's the least likely occurrence that would have been observed; if there was really no association between mesothelioma and crocidolite, then this observation would have been the least likely distribution of the data, and it had a probability of one in thirty-two. So that is the significance level of this finding, after allowing for chrysotile.

Could I have the next slide, please.

MR. LASKIN: I take it, just so we're clear, from your article, is that information in another form contained at table 7?

MR. BERRY: Correct; yes. That information could be extracted from table 7, and it's given in the text but not as an actual table. And in the actual text, we talk of a significance level of point 06, which is a two-sided significance level. The one I gave you there was a one-sided one, which has

MR. BERRY: (cont'd.) therefore got half the value.

Perhaps we could skip the next few slides; if you keep going on. Keep going; keep going; right. Those last few slides which I skipped over were a refinement of the lung cancer deaths -- lung cancer case control study, looking at dose-response relationships. And it won't surprise you that we found little evidence of - well, perhaps it would surprise you; I'll withdraw that remark.

In fact, we found no evidence of a dose-response relationship for lung cancer, and the main reason was that the exposure, in cumulative terms, were very low, in historical terms; in fact, only four out of a hundred lung cancers had reached a hundred fibre years per ml.

On this slide ---

DR. MUSTARD: This is all referring to a group of people exposed a long time ago?

MR. BERRY: Yes -- well, between twenty and forty years ago.

DR. MUSTARD: There is a problem in the quality of the diagnostic applications of medicine to those cases, then, because the standards of medical diagnosis, even of post mortems, have been shifting over that time period. Do you make any allowance for that?

MR. BERRY: Well, we make allowance for it in the sense that all the comparisons are in comparison with the national population, and the national population at that time; so we are, in effect, assuming that the level -- the standard of diagnosis on death certificates, in any year, is the same for people at this factory than for elsewhere. So we are allowing for changes in time, but on the assumption that the changes in time are occurring at the same -- of the same form for people in

MR. BERRY: (cont'd) this factory as for the population at large.

DR. MUSTARD: Do you know if there's any variation in the United Kingdom, particularly that historical period, between the precision of the diagnosis in different sectors of the country, if there's any linkage of that to where the asbestos would be located?

MR. BERRY: No; I can't really answer that, I'm afraid.

This slide summarizes a number of studies which I think you've already come across most of them. It looks at the standard mortality ratio for lung cancer at a hundred fibre years per ml. The Canadian study of McDonald, which you'll hear about next week -- but there, he found a dose-response relationship but the doses were very big, and, at a hundred fibre years per ml, the SMR was only one hundred and four; and this is similar to what we obtained in this friction materials, where it was a hundred and two.

Now, given two other groups of textile workers, now, we were looking at chrysotile, and so I'm looking at chrysotile studies here, the second one, which is the asbestos textile -- it's the same one as I've been talking about earlier; the mortality aspects I haven't been involved with; they have been done by the Oxford group over a long period of time, starting with Dahl's paper of 1955. The latest paper by Peto gave an estimated SMR of one hundred and sixty; and that was a factory which was predominantly chrysotile, but where crocidolite was in widespread use.

We have confirmed that recently, from post-mortem analysis, mineral analysis, of lungs, using electron microscope techniques; there is crocidolite in the lungs of those cases at a higher rate than would be expected in the absence of any

MR. BERRY: (cont'd.) occupational exposure.

MR. LASKIN: Just to interrupt you for a moment, are those figures all extrapolations, in the sense that there were no even estimated measurements at that level?

MR. BERRY: They are based on a linear dose response between lung cancer and cumulative dose.

MR. LASKIN: And is the figure of a hundred fibre years per millilitre an extrapolation on the linear dose response curve?

MR. BERRY: Correct; yes. For example, Peto's study, he had data at two hundred and fifty fibre years per ml.

The next one is a study of Dement, and that's not an extrapolation; he did have data down at that level. And I think this one stands out. I understand Dr. Weill discussed this with you yesterday; he knows far more about it than I do.

But this is a finding which surprised everybody when it came out, and I think, at the moment, it's not quite clear how it fits in with other findings.

Getting back to the friction materials, you'll notice I've put at the bottom the upper limit of one hundred and eighty. Because the exposures were so low, we were unable to estimate the dose response with precision; and, in fact, our upper ninety-five competence limit is as high as the level obtained by Peto; so those results are not necessarily contradictory.

Taking Dement's study into account as well, it looks as if textiles is a more dangerous operation than mining or milling, or friction materials. Could we have the slide off, please.

It came out of that study that the mesotheliomas were mainly associated with the minority of workers who'd been exposed to crocidolite. As I mentioned, other data has been

MR. BERRY: (cont'd.) given on this subject.

If I can just mention one example from Alison McDonald, which she presented at a meeting in Helsinki last week, where she looked at three factories in the United States; two of them, they were working with chrysotile, and one of them with amphibole -- I've forgotten whether it was crocidolite or amosite, offhand; but, anyway, it was amphibole.

And out of over two thousand deaths in the chrysotile workers, she had one mesothelioma; and, in contrast, in fourteen hundred deaths in amphibole workers, there were eighteen mesiotheliomas. These are rates of 0.4 and 12.6 per thousand deaths; so it was a factor of about thirtyfold there.

DR. UFFEN: What kind of a factory was it; was it a textile?

MR. BERRY: Oh; the ones that Alison McDonald was studying? One of them was a textile factory; I've just forgotten what the other one was -- oh; the other one was a friction materials, similar to the one I've been studying.

MR. LASKIN: Do you know whether the figure of fourteen was pure crocidolite, or a mixture of crocidolite and chrysotile exposure?

MR. BERRY: The amphibole one, where it was eighteen out of fourteen hundred; I don't recall offhand, no.

Right. I'm not saying, of course, that mesotheliomas don't occur as a result of chrysotile exposure, but just that it is a rarer event. Mesotheliomas can certainly be produced by chrysotile in animals; and my colleague Dr. Wagner has done quite a lot of animal experiments, and has shown that chrysotile can produce mesotheliomas at quite a surprising high rate, when injected into rats.

The actual samples that have been injected have been rather fine samples, milled more than what is usually used

MR. BERRY: (cont'd.) in industry, I believe. But it does show that, if chrysotile is processed to produce fine samples to separate the fibres sufficiently, then certainly there is a potential for producing mesothelioma.

Well, I think, Mr. Chairman, I've been speaking for long enough; possibly too long. I did have a couple more slides to show, but I think I'll leave those and maybe you'll come onto them in the questions.

DR. DUPRE: Thank you, indeed.

MR. LASKIN: Thank you very much, Mr. Berry.

DR. DUPRE: Do you wish to have a glass of water, or ...

MR. BERRY: If I could have a refill; yes, thank you.

MR. LASKIN: Perhaps the witness would appreciate five minutes for a break, and I don't know what the pleasure of the Commission is -- shall we then go till one o'clock and break for lunch, or ---

DR. DUPRE: The idea is, we would break at 1:00 and return, say, at 2:15. We'll take a five-minute break now.

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MR. LASKIN: Mr. Berry, can I just make certain that I understand the various plants that you and your colleagues looked at; and, as I understand it, one plant you looked at was the Rochdale plant, and that was the operation on which the 1968 British standard was developed.

MR. BERRY: Yes; that's right. The Rochdale plant I've only looked at from the point of view of morbidity; not of

MR. BERRY: (cont'd.) mortality.

MR. LASKIN: Those are -- you looked at in respect of asbestosis morbidity, and those are articles at tabs 7 and 8.

MR. BERRY: Yes.

MR. LASKIN: And is that a textile plant?

MR. BERRY: That is textiles, yes.

MR. LASKIN: And that's the same plant that Peto looked at in his figure you showed on that slide a little while ago?

MR. BERRY: That's right; yes.

MR. LASKIN: Then, as I understand it, you very recently looked at, and presented at Cardiff, a friction materials plant.

MR. BERRY: Correct; yes.

MR. LASKIN: And the third plant you looked at, as I understand it, was also a textile plant but was in London?

MR. BERRY: It was in London; it wasn't a textile plant, although there were some textile operations going on. That is the one in tab 6.

MR. LASKIN: Six. And also, I take it, tab 2; we looked at smoking.

MR. BERRY: Correct; yes.

MR. LASKIN: And what kind of asbestos operation was that?

MR. BERRY: There were various departments, but they were involved -- there was a brake-lining department; there was rubber jointing. The brake-line industry was chrysotile; rubber jointing use crocidolite and chrysotile.

There were textile departments; there was a mattress-making section. In the carding, weaving, and mattress-making, crocidolite had been used -- no, sorry; crocidolite had

MR. BERRY: (cont'd) been used in spinning and doubling, and the chrysotile and amosite for carding, weaving, and mattress-making.

The different types of asbestos were so mixed up that it wasn't possible to separate groups out and say they just worked with one particular kind of fibre.

MR. LASKIN: And in this study which is at tab -- you refer to in your articles at tabs 2 and 6, there's a group of persons who you call, in your article, laggers, which I take it is a British term, and you've told me is an insulation worker.

MR. BERRY: Yes.

MR. LASKIN: So the laggers in your study are basically like Dr. Selikoff's insulation workers?

MR. BERRY: That is so; yes.

MR. LASKIN: And you also use the term "mates." What is that?

MR. BERRY: Oh -- laggers and their mates we use; is that it? Yes. A mate just refers to an assistant, who might not be as well qualified, so you'd have a lagger and his mate would be helping him, but maybe he wasn't -- didn't know enough about it to do the job by himself.

MR. LASKIN: And, as I understand it ---

MR. BERRY: But their exposure would be similar.

MR. LASKIN: --- one of the significant features of laggers, I take it, is that they may not have spent all of their time at the particular plant that you're investigating?

MR. BERRY: No; some of the laggers had been exposed to asbestos before they joined this particular factory, and laggers are -- they don't work in the factory; they visit other places where insulation is being fitted. So they travel in their work and, because of this, they tend to take jobs with

MR. BERRY: (cont'd.) other firms who are also doing lagging, as opposed to the ordinary person working in the factory who, if they change their job, they would go to some other factory in the same town, probably, which might have been doing quite different work not involved with asbestos. But the laggers tend to be more specialized, and a lot of them certainly change firms but still do lagging.

MR. LASKIN: We've heard some evidence to the effect that, certainly insofar as insulation workers in North America are concerned, that they perhaps are subjected to rather more intense burst, or doses, of asbestos, but for shorter periods of time than other kinds of asbestos workers. Are you able to say whether that situation prevails with respect to your laggers?

MR. BERRY: Well, not from direct knowledge of seeing this work going on, or from any dust measurements, but it certainly fits in with the pattern of work, because they would -- as part of the job of applying insulation, part of it would be getting things ready, and then they would fit the insulation and there would be spraying going on sometimes; and they obviously wouldn't be doing the spraying part for the whole day, in contrast to somebody working in textiles, on a carding machine, where the carding machine would be running non-stop, except when there was a breakage.

MR. LASKIN: Were you able to make any assessment as to whether the dose-response relationships for laggers were different than for other kinds of asbestos workers; or, to put it more generally, is there some -- is there some difference in hazard when a person gets a short, intense dose of asbestos as opposed to the same dose but over a longer period of time?

MR. BERRY: Well, we weren't really able to look at that, because we didn't know the doses; we only knew them

MR. BERRY: (cont'd.) qualitatively. And the laggers would certainly be classified in the severe group; we'd put them into the severe or the moderate, but we'd put them into the severe. But we don't know the relative dust levels, and we don't know the lengths of time that -- for lagging -- when the dust levels were at the high level.

MR. LASKIN: I want to discuss with you just for a few moments your work at Rochdale. Can I just get -- there seem to be three terms which -- and this is your morbidity study, and there seem to be three different indices that seem to be prevalent in your studies, and I take it one is crepitations, and is that the same thing as rales?

MR. BERRY: Yes.

MR. LASKIN: And can you just tell us very briefly, what is that?

MR. BERRY: That is a sound heard through a stethoscope in the chest. As I'm not a medical man, I can't really elaborate on that, but ...

MR. LASKIN: Do I take it that that's not an index that's necessarily cause specific to asbestos exposure; I mean, it may result from some other kind of exposure?

MR. BERRY: I think that is the case, yes.

MR. LASKIN: And is it also fair to say that, simply because a person doesn't have crepitations doesn't necessarily mean that there isn't some evidence of asbestosis which may show up in the X-ray, or something like that?

MR. BERRY: Yes; that could occur, yes. The crepitations -- if I can go back to what we're talking about here -- were in the base of the lung; we do distinguish between those heard in the base and elsewhere. And it's defined in a little bit more detail in the paper.

But if you look at figure 1 in this reference --

MR. BERRY: (cont'd.) it's number 8, isn't it; number 8 -- look at figure 1; then you can see that there were some people who were classified as possibly having asbestotic who didn't have crepitations. And that would be done on the basis of the X-ray and lung function.

MR. LASKIN: Am I correct that it was crepitations, or rales, which was the index that was used to develop the standard in 1968?

MR. BERRY: Yes.

MR. LASKIN: And ...

MR. BERRY: You'll also, going back to your last question, see from that figure, as well, that there are quite a number with crepitations who were not in the possible asbestosis category, and this is because they didn't have any supporting signs.

MR. LASKIN: Do I take it that what was attempted in 1968 was to develop a level where it was probable that the risk of contracting asbestosis would be less than one per cent?

MR. BERRY: Yes.

MR. LASKIN: And asbestosis was defined as, I take it, developing crepitations or rales, for the purpose of developing that index, or that standard?

MR. BERRY: That is so. I can't recall whether we actually called it asbestosis; I think we called it early signs of asbestosis.

MR. LASKIN: And when you then subsequently did your own work in 1979, the index that you used was possible asbestosis?

MR. BERRY: That is right; yes. Or that is the main one; as you say, we use crepitations as well.

MR. LASKIN: Did ... Dr. Uffen asked a question. I take it, it was ultimately a matter of clinical judgment, but

MR. LASKIN: (cont'd) did it involve a combination of X-rays, lung function tests, and the existence of crepitations?

MR. BERRY: I'll just find what we actually did.

Yes; it was based on crepitations, X-rays, and lung function, in particular, falling gas transfer, and restrictive changes in lung volume or ventilatory capacity.

MR. LASKIN: You've already told us that, as far as you and your co-authors were concerned, you never intended that paper to be a paper by which one would set standards.

Can I ask you this: was there anything, when you looked at the index, possible asbestosis, was there anything implicit in that that you were drawing any connection between asbestosis and lung cancer or, indeed, any other kind of cancer?

MR. BERRY: No; we were simply looking at disease as measured in life.

MR. LASKIN: Dr. Weill put to us a thesis to the effect that, determining whether any particular lung cancer is attributable -- in determining whether any particular lung cancer is attributable to asbestos exposure, it's a fairly reliable guide to look at whether the person has evidence of asbestosis. I think that's a fair summary, and I'm just wondering whether there was any of that notion in either the British approach in 1968 or, indeed, the approach that you took in 1979.

MR. BERRY: Yes; I think there was a little bit of that in 1968, because if you -- there is a section there called "Cancer," and although it says there isn't sufficient data to carry out a similar analysis for cancer, it states that if one controls asbestosis, then one probably controls cancer.

I mean -- can I ask if Dr. Weill -- in Dr. Weill's hypothesis, was he referring to asbestosis previously determined in life, or a post mortem, because the risk can be quite

MR. BERRY: (cont'd.) different.

MR. LASKIN: At the risk of doing him an injustice, I had understood his thesis to be that a given fibrogenic dose would also be roughly equivalent to a carcinogenic dose.

MR. BERRY: I see; yes.

I mean, we certainly weren't -- the work we did did not depend on any such hypothesis.

MR. LASKIN: Right.

Now, as I understand it, when you came to do your work at Rochdale, in part of it, as I read your article, you seemed to indicate that the dust-measurement problems that are inherent in all of this would have had some considerable effect on even the British approach in 1968.

And as I understood what you -- and correct me if I'm wrong -- as I understood it, you suggested that their dust-exposure measurements in 1968 may have been over-estimates.

MR. BERRY: Yes; yes, that is so. I don't think that was principally because of the problems of measurement, though. In 1968, they didn't have detailed job histories available. What they did have was that a person was in a particular job and that he'd been in the factory for a certain length of time. And it was assumed, at that time, that he'd been in that job for the whole period.

But when we did the 1979 study, we took the occupational histories in great detail and found a lot of people had changed jobs; and, as a result of this, they spent some of the time with lower dust concentrations and, therefore, their cumulative exposure, in whatever way -- however you define it -- would be less.

MR. LASKIN: Can I take you to tab 8, which is -- you may have it in front of you.

MR. BERRY: This is British Journal of Industrial

MR. BERRY: (cont'd.) Medicine, is it?

MR. LASKIN: Yes. Can I take you to page 105.

MR. BERRY: Right.

MR. LASKIN: And can I take you to the lefthand side of the page, and under the heading "Relationship of Signs to Cumulative Dust Exposure" ...

MR. BERRY: Yes.

MR. LASKIN: Now, as I understand what you are suggesting there, if you took -- if you took the same approach that the British took in 1968 on the basis of your own data ---

MR. BERRY: Right.

MR. LASKIN: --- and your own summaries, that prevalence of one per cent would come not at a hundred or a hundred and twelve fibre years, but at forty-three fibre years per cubic centimetre?

MR. BERRY: Yes.

MR. LASKIN: So that if you had gone the next step, as presumably the British had in 1968, your standard would come out at one rather than the British standard of two, I take it, or, in fact, even less than one?

MR. BERRY: Yes; that would have been the case, yes.

MR. LASKIN: And the second -- the next paragraph down, I take it, just simply indicates that, if the British had applied more accurate dust levels, that even their own approach would have produced a standard less than two in 1968; is that the thrust of the next paragraph?

MR. BERRY: Yes, yes; that is so.

MR. LASKIN: Now, I also gather, from looking at this same article, that one of the ways that you got your fibre measurements was to do some conversions -- and we touched on this briefly -- between particles and fibres?

MR. BERRY: Yes. We didn't actually do them as conversions, but it amounted to the same sort of thing.

MR. LASKIN: Did you do simultaneous sampling for particular years and then relate whatever correlations you had back to previous years when there had just been particle counts?

MR. BERRY: No. What was done was, that change-over was in 1961; they started using -- they started measuring fibres in 1961. Previously, the thermal precipitator had been used, and that had been used over a ten-year period.

And what we did was, we took the ratio of the thermal precipitator count for 1952, and with that for 1960 we assumed, in effect, that the 1960 thermal precipitator count was equivalent to the 1961 fibre count. And we then multiplied up by the ratio of '52 to '60 to effectively do a conversion -- I'm sorry; perhaps that's not very clear. Perhaps I can write it down.

MR. LASKIN: You may have to speak up a bit, just to get this on the microphone, Mr. Berry.

MR. BERRY: I've got fibre count in 1961; we've got thermal precipitator in 1960, and we've got thermal precipitator in 1952; we used this ratio, and said, in 1952, conditions were more dusty in 1960 by whatever ratio that is -- by a certain factor.

We then said, in 1961, there were so many fibres -- these two dates are very nearly the same, so we'll assume that the thermal precipitator in 1960 was the same as the fibres in 1961. So, by doing this bit of arithmetic, end up with the fibres for 1952; the estimated fibres for 1952.

MR. LASKIN: I take it you did that kind of calculation for various jobs within the plant?

MR. BERRY: Yes.

MR. LASKIN: And, as I read your article, came up

MR. LASKIN: (cont'd.) with ratios that varied between one point four and three point four?

MR. BERRY: That's correct; yes.

MR. LASKIN: Were there any confidence levels or correlation coefficients, or anything like that, put on those figures?

MR. BERRY: No. They were based on a lot of samples, though.

MR. LASKIN: Would it be fair to ask you, as a statistician, how confident you yourself are of those conversions, or those ratios?

MR. BERRY: Well, I'm sure it's fair to ask me; I'm not too sure that I know the answer. But of all of these things -- what we were doing was what we considered the best thing one could do with the data; but they were all based on a lot of measurements. And this factory was being measured continually, so they are based on hundreds of measurements.

But the key thing is whether the composition of the dust had changed between '52 and 1960, so that a given number -- well, in effect, the conversion from particles to fibres might have been different in '52 than it was in 1960; and, if it was, we would have completely missed that -- we wouldn't have had the data to get at it at all.

MR. LASKIN: It's implicit in the way you approached it that there's some relatively stable dust composition over time, and some given dust cloud or ...

MR. BERRY: Yes. Of course, they were doing similar types of operation; I mean, the main caveat to have is whether the asbestos, as it was being delivered to the factory -- whether that composition was the same; and I don't know the answer to that.

MR. LASKIN: It might be, Mr. Chairman, a

MR. LASKIN: (cont'd.) convenient place to stop, since it's coming up to one ---

DR. DUPRE: Right; we shall rise then, to reconvene at 2:15. Thank you.

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DR. DUPRE: Counsel, will you proceed, please?

MR. LASKIN: Thank you, Mr. Chairman.

MR. LASKIN: Q. Mr. Berry, just to finish off the matter of these fiber ratios, I take it the lesson out of all of this is that these ratios can vary quite considerably even within one asbestos plant?

THE WITNESS: A. Yes, indeed.

Q. Presumably, then, can vary from plant to plant, and can vary from whatever part of the asbestos industry you are talking about to whatever other part you are talking about?

A. Yes, I think it would be a lot more uncertainty if you took conversions in one plant, established at one plant, and applied them to a different site of operation. That would be quite a hazardous situation.

Q. In any event, when you did your study of Rochdale, and correct me if I'm wrong, but you, in any event, attempted to correct one of the problems, a number of the problems with the previous assessment of Rochdale that led to the British standard, and one of them was dust measurement?

A. Yes.

Q. What other deficiencies in the original assessment did you attempt to correct in your updated work on Rochdale?

A. Well, we looked at occupational histories a lot more closely. That was one thing. Each individual was gone through, factory records at shop-floor level, to find out which job they were doing when. That was got out in great detail.

The second was, the original study was based on people who were actually working there on a certain date. In this later study we looked at people who had left also, and

MR. BERRY: (cont'd.) about seventy percent of those who had left agreed to come back to the factory and have an x-ray taken. This was quite an unusual departure, because normally in Britain a medical officer in a factory has no responsibility for people once they have left. It took quite a bit of persuasion on his part to persuade the management to let him look at people who had left. So we were looking at people who had left as well, and that was important because they might have left because they got some ill effects. In fact it came out...I don't know whether I can find the place in the paper...but that was the case.

I can't find it. If you read through the paper, you'll find that that is stated somewhere.

I think probably they were the main...well, then the third one was using more than crepitations...using x-rays and lung function as well.

Q. I suppose one additional advantage you would have had, simply by reason of lapse of time, was that you would have been in a better position because of latency?

A. Well, that is so in one sense, but not in another, because previously we hadn't distinguished between people who started before and after 1951. Previously we had used a guesstimate of early dust levels. We decided not to use it, so in one sense we reduced the period of time. But it was...the dust levels were more accurately known, so we did include it.

Q. In your study, what would be the minimum amount of time that any one employee in your cohort would have from time of first exposure to the time that you made your assessment?

A. Well, assessments were, medical assessments were made throughout the working period of these men. Once they were doing a job which was possibly hazardous, they were being seen regularly by the medical officer, so that we would

A. (cont'd.) detect that he got crepitations on a certain date. We didn't just rely on him having crepitations when we did the study. We looked back in the files to find out when he had first had crepitations recorded, or when he had first been considered a possible asbestotic.

Q. I probably didn't put my question properly. Let me try it again. What I'm really trying to get at is, the period of time between the date when your calculations appear and the date when a particular employee was first exposed to asbestos, what would be the minimum period that that might be?

A. A minimum period to some sort of symptoms? I think there was one about eight years after first employment.

Q. As I understand it, your followup period was ten years?

A. The followup period was longer than ten years. They had to be in the factory for ten years.

Q. They had to be in the factory for ten years?

A. Yes.

Q. All right. Could the period of time that they were in the factory...let me try this again...they are in the factory for ten years, say up to 1965...

A. Yes.

Q. And you looked at them..was your cutoff date 1965, and then your followup period after that?

A. No. The cutoff date was...there was no cutoff date except that...let me just look at that again, if I may, and make sure I give you it correctly.

Ah yes, the cutoff date was the 31st of December, 1972. You had to have completed ten years by that date. So it included some people who weren't in the original study because they had completed the ten years service after the 30th of June, 1966, but before the 31st of December, 1972.

Q. Then were the calculations made as of 1975?

A. Yes.

Q. So I guess the answer to my question is that the minimum period from first exposure for any particular employee would be thirteen years. If he happened to have been employed...

A. Yes.

Q. ...in 1962, you would still trace him in 1972 and then he would have a further three years?

A. Yes. Yes, I'm sorry I misunderstood you.

Q. Just looking at...I think you say somewhere in your article, that the mean latency period for this cohort was about sixteen years?

A. Yes.

Q. Is it...are you still following this particular cohort?

A. No, not at the moment.

Q. Is it fair to say that there are likely to be more effects as time goes on?

A. Yes. Yes, I think that undoubtedly would be the case, and if you remember, the graph that was up from the New York paper with the dotted lines increasing. It's that sort of situation you would expect to be happening.

Q. I take it your mean latency period has not yet reached what might be the peak latency period for this particular cohort?

A. I think probably that is the case, yes. Of course, we tried to allow for that by introducing these measures of exposure which allow for development of disease after our period of observation.

Q. Could I just take you to....I can't recall the reference, but it seemed to me that at one stage in one of your articles you indicated that there had been an absolute increase in the prevalence of crepitations from about six percent to twenty-two percent?

A. That's right, yes. It's in either the paper we are talking about or the New York paper, yes.

Q. All right. So does that then mean that nearly one-quarter of this cohort that you studied would have had some evidence of asbestosis?

A. Well, they would have had crepitations, yes. Yes, that's in the New York paper, table one.

Q. Which paper is that now?

A. That's number seven.

Q. So that's at which page, Mr. Berry?

A. Page 188.

Q. I see. Then twenty-nine percent of those who in fact had left would have crepitations?

A. Yes. That's right. Yes. That takes it to the point we were discussing a few minutes ago, the importance of having these exworkers included. There was evidence there that people were...the people who left had more crepitations than those who had stayed.

Q. Can I come back just for a moment to dose-response in relation to this study...and I know you dealt with it at some length this morning. Could I come back to this table of the various curves, which is found at page 190 of tab seven?

A. Yes.

Q. Did I understand you to say that all of the various curves that you had drawn here fitted your data equally well, with the exception of the cumulative dose curve?

A. Yes, roughly speaking. They didn't all fit equally well, but they all gave satisfactory fits. It wasn't possible to exclude them.

Q. Was there any one of these curves that fit your data better than any others?

A. Not to any significant extent, no.

Q. There is one thing I don't think I am

Q. (cont'd.) understanding completely, and that's the...why the cumulative dose curve becomes horizontal at twenty years.

A. Well, it's based...the model using cumulative dose, it's the dose-response relationship is the prevalence of disease is a function of cumulative dose. Once exposure is stopped, then cumulative dose doesn't change its value, and therefore any function of cumulative dose doesn't change it's value, and so the prevalence remains at the same value. That is, in fact, one of the reasons for thinking a priori whatever the data show. I think in a priori the cumulative dose is not very realistic.

Q. Wouldn't there be some people who had accumulated a specific dose, but which...whatever prevalence index you are using, that prevalence index wouldn't show up in that particular person until thirty years as opposed to twenty years?

A. Yes. Yes, that's right. And that's the point, you see. Cumulative dose...if we assume cumulative dose was the correct measure, then we are effectively assuming that the disease does not...that such a prevalence doesn't exist, and that's why we have got to have one of these other relationships where we are allowing prevalence to increase.

So this person who did twenty years exposure and then left with no disease, and as you put it, he might develop a disease ten years later, he is one of the people on these graphs, these upward-moving graphs, which is making the move upwards.

Q. Why isn't he...that's where I'm having the problem. Why isn't he part of the cumulative dose graph?

A. Because we are saying that cumulative dose has got certain disadvantages, one of which is that when the exposure occurred doesn't matter. The other models are saying

A. (cont'd.) the longer ago the exposure was, that increases the chance of him getting disease. Now, if he works in the factory and then leaves, the time continues to accumulate, so that ten years later if you look back in time, you give a bigger weighting to his exposure while he was in the factory. In another ten years, you give a bigger weighting still. So all the measures except cumulative dose are continuing to increase even though the man isn't exposed. Because they are continuing to increase, this means that under this modelling situation he still is at risk of developing disease.

Q. The model of cumulative dose assumes that the dose comes into you and instantaneously leaves you?

A. Effectively, yes. I mean, I don't think anybody had ever looked at it that way. But if you start building these models with elimination in, then you find out that mathematically that is the case, yes.

What we are talking about, elimination, of course, I'm using in this paper elimination to mean that it's got no activity anymore. It may be that it's engulfed by macrophages and rendered inactive. It might still be there, but it has somehow been rendered inactive. So I'm using elimination as a blanket word to mean biologically inactive, as well as actually. physically being removed from the lungs.

DR. UFFEN: Can I ask a supplementary here? I understand that for the case of assumed exponential behaviour, that is the rate of change in proportion to the amount present, that's an assumption that's probably as good a one as you can make. But suppose it were linear. Would the same limits apply if it were not an exponential thing?

MR. BERRY: By linear, you mean just a certain amount that is eliminated each year, irrespective...

DR. UFFEN: I just used that because it's a really simple one to...

MR. BERRY: Yes. Well, if the elimination was in a different pattern, it would alter these curves, yes. You know, without actually doing the calculations, I don't know how...

DR. UFFEN: Mathematically they might not proceed to a nice limit like you have here, nor the half-life times the dose approaches the two limiting cases...in your appendix?

MR. BERRY: Yes, I see.

DR. UFFEN: Do you know what I mean?

MR. BERRY: Yes. I still think they would probably be the limit, because there are the limits of no elimination and very quick elimination, so I think whatever model of elimination you have.

DR. UFFEN: It's probably the only one that...the intermediate cases would depend on the assumed method of elimination?

MR. BERRY: Yes.

DR. UFFEN: Could I pursue this, because there is a question that comes out of it?

MR. LASKIN: Yes, I would be delighted if you would.

DR. DUPRE: Dr. Uffen, just before you do, could you specify for the record what appendix?

DR. UFFEN: I'm talking about number eight. Seven and eight are related, but I'm talking about eight. In the appendix to eight there is a mathematical analysis which relates the decay constant lamda, the half-life T, to the dose, and demonstrates mathematically the relationship with these two limiting cases.

Okay?

MR. BERRY: Mmm-hmm.

DR. UFFEN: There is another possible one which intrigues me and that is, instead of the simple assumption that the rate of change is proportional to amount present, in certain kinds of systems, biological systems, if you have a very, very large amount, an overwhelming amount, you don't get straight exponentials. You get some other thing, a logistic curve of some kind.

MR. BERRY: Yes.

DR. UFFEN: Have you toyed with that kind of assumption instead of the exponential one?

MR. BERRY: No, I haven't tried that.

DR. UFFEN: Do you know if anybody has?

MR. BERRY: No, I don't think I do.

DR. UFFEN: The thing that's in the back of my mind is that, the special case of a worker who is subject to quite high exposures for short periods of time, and whether your very nice analysis of the importance of weighting comes into question for that special case of short, but intense, exposure?

MR. BERRY: Yes. Were the rate of elimination marked different. Yes.

Yes. I mean, this analysis is...I know most people find it very complicated, but in essence it's quite simple. It's just assuming one rate of elimination, and if we then start saying the rate of elimination might vary from one individual to another, it might vary, as you are suggesting, according to the intensity of the exposure. Then, of course, that is a lot more complicated and the conclusions will be even more uncertain than they are already.

I think there has been quite a lot of work done on elimination, but I can't recall whether it's with fibers or with particles...using radio-optic techniques and labelled, probably, with particles.

And people have suggested that the elimination is

MR. BERRY: (cont'd.) a mixture of exponentials. There is one component which is elimination over a very short period of time, say a week, and then you've got another which is elimination over two or three months, say, and then you've got a longer-term elimination. Really what I am concerned with is the longer-term one.

DR. UFFEN: Is there biological evidence for this type of behaviour, rather than just a mathematical assumption that a statistician has to make?

MR. BERRY: I don't think anybody has made observations of this sort in man over the period we are talking about, you know, several years.

DR. UFFEN: Could you do it with animals?

MR. BERRY: Yes, it could be done with animals. I don't know that it has been.

DR. UFFEN: Suppose we did it with animals and it turned out fortuitously to be pretty close to the exponential one, and they couldn't find a half-life. Could you make the leap for a half-life as measured in an animal, use those figures for humans?

MR. BERRY: Well, I don't know whether I'm competent to answer that question, but I would think it would be quite...it would involve quite a lot of assumptions.

There have been studies done in animals, of course, measuring elimination, but we've done studies ourselves at the pneumoconiosis unit, but we haven't...and we observed that elimination does take place, but we haven't done studies to actually develop the whole elimination curve.

DR. UFFEN: May I pursue one more? This is as good a time as any, because it won't make us come back to do it all over again.

I'm curious about the time here. Twice, at least twice, we've had put in front of us the importance of time.

MR. BERRY: Yes.

DR. UFFEN: First of all it was the delay time or I've heard the term 'time to tumor' sort of thing.

MR. BERRY: Yes.

DR. UFFEN: Now we are talking about another time, an elimination time.

MR. BERRY: Yes.

DR. UFFEN: Is it reasonable to think that there might be a relationship between these two times, determined by the biology? That if you measured one, you might be able to predict the other?

MR. BERRY: I don't really know. Obviously I'm no biologist, so...but I've always taken it time to tumor is something to do with time to some biological process to take place, starting, if we say a tumor starts from a single cell, then it needs time to grow and be detectable. There was a paper by a chap called Geddes in, I think it was the British Journal of Diseases of the Chest, 1979 or 1980, where by some argument on cell-doubling time and that sort of thing, he showed that a lung cancer couldn't be detectable for about ten years from its initiation.

That is, we are just saying when it's detected on x-ray or by a man complaining of symptoms, its size is measured on surgery or post mortem, it's a certain size which consists of so many cells, which you calculate. Starting from a single cell and with a known doubling time, it has taken ten years to get there.

I'm not competent to say whether his argument is biologically valid, but, you know, it seemed quite a reasonable way of proceeding and it seemed to fit in, certainly with asbestos where these things are never detectable before ten years.

DR. MUSTARD: Can I just interject, counsel?

MR. LASKIN: Yes, go ahead.

DR. MUSTARD: There is considerable animal experimentation in carcinogenesis that shows a very good correlation between dose and time to response, which...and indeed I think that's one of the reasons why these massive doses of saccharin in the testing took the time to demonstrate to tumor doesn't take a long time...in view of that animal data, wouldn't you be a bit concerned about the kind of assumptions of doubling time for ten years?

Are you familiar with the animal experiments...

MR. BERRY: I'm not familiar with all the animal experiments with different materials, but certainly we've done animal experiments with different doses, and this is in paper number...it's mentioned in paper number seventeen, which Mr. Laskin has a note of but isn't on the file.

We had a range of doses of sixteenfold, and we couldn't detect any change in latency with dose. Of course, it is difficult to detect that sort of thing unless you have a very big experiment, which we didn't have.

But you've got to bear in mind that if the dose is high, you are going to get a lot of tumors, and therefore these will become detectable earlier than if the dose is low and you've got fewer tumors. But I can't comment on whether the literature you are referring to, whether this effect is present or not. But there is a possibility of an apparent association, an apparent reduction in latency with dose which could in some senses be an artifact.

MR. LASKIN: Q. Is there any work in the human studies that deals with that particular issue, the relationship between dose and latency, so far as your work?

THE WITNESS: A. Yes. There is a paper by Seidman and others in the New York Academy of Sciences, the 1979 issue, the same issue as my papers are in. He puts forward the hypothesis that the latency is reduced with higher exposures.

A. (cont'd.) But I'm not too sure whether it is established or not, or whether it could be a sort of artifact that I have been speaking of.

You can detect the same sort of thing in, if you look carefully, in some of the data I gave this morning on ... from the study in the London factory where, if you remember, I showed you low/moderate and severe, less than two, greater than two years.

There is some evidence of an excess of lung cancer in the low/moderate groups which has only emerged in recent years. That is, it wasn't present in the first analysis ten years ago. So you could say this is an argument for...it only now becomes apparent because the latency is, say, thirty years instead of fifteen years...but again, we've got the problem of building up sufficient numbers for something to become significant.

MR. LASKIN: I think Dr. Uffen has a followup question.

DR. UFFEN: I think I have a supplementary...it's related to the same diagrams, and again it's partly to make sure I understood this right.

I gather from your papers seven and eight, that you might say a preferred method would be to use the no-elimination model, if you regard preferred to be prudent?

MR. BERRY: Yes.

DR. UFFEN: Is that...?

MR. BERRY: Yes. Well, I don't think we actually said that in the paper.

DR. UFFEN: No, but I'm trying to extract it.

MR. BERRY: Yes. If you are saying that we've got this range of estimates, from the data we can't choose between them, therefore we will choose the one which offers...we will choose the most pessimistic model in order to give the work force the greatest possible protection, then that will be so. Yes.

DR. UFFEN: If we do that, then...in your paper number eight, there is this rather interesting figure four that has been referred to which has the plot of crepitations and possible asbestosis against cumulative exposure.

MR. BERRY: Yes.

DR. UFFEN: Presumably you could plot that over again, instead of using cumulative exposure, using the most pessimistic case?

MR. BERRY: Correct, yes.

DR. UFFEN: It's hard for me to visualize what it would do to the diagram. Would it make...

MR. BERRY: Yes.

DR. UFFEN: Shift those curves around very much?

MR. BERRY: Well, you see the whole scale will become different, because we are then talking...we are bringing in another variable, another time component, the units change, we have time squared instead of time. I think the best way of looking at it is probably on table eight on page 107, and comparing...if we just stick to the top part of that table, the one that's called logit model (no lag)...and you could look at, say, the estimated concentration to give a one percent prevalence, which is one point one with cumulative dose, and that refers to the diagram, not...well, I'm looking at the forty year column.

DR. UFFEN: Right.

MR. BERRY: That refers to, say, a cumulative dose of forty-four, given one percent. With the most pessimistic model it would be 0 point three. So it's a factor of four in the concentration in the air.

DR. UFFEN: Now, can I go one step further. Would it make it possible or not, and easier, for someone who could measure crepitations, to make an early diagnosis?

MR. BERRY: Well, ...

DR. UFFEN: Or would there be no real change in the discrimination?

MR. BERRY: I mean, it's from the figure, figure number four, it's clear that the actual index of response that we use isn't all that critical. It's only a factor of, say, two to one, as compared with a factor of four to one we just mentioned.

So that certainly crepitations would appear to be a reasonable thing to use, and it would just mean we were, say, doubling the prevalence. So we could adjust our dose accordingly.

MR. LASKIN: Q. Just while we are on this table eight, at page 107, if we move over to the numbers on the righthand side, do I take it there you are attempting to estimate what an average fiber standard would have to be to protect against more than one percent prevalence of possible asbestosis after thirty, forty or fifty years of exposure?

THE WITNESS: A. Well, that is effectively so. We are attempting to estimate that sort of thing, but by this stage we have got to the situation where we realize we can't estimate it. We can only give a range of values, as indicated by the values in each set of five.

Q. But the range of values, again, depend on which one of your...

A. Correct.

Q. ...curves you use?

A. Yes. So we are not attempting to select one of these figures and say that is the standard. We are attempting to show figures which will enable those people whose business it is to set standards, to give them some guidance, to show them what is involved.

Q. I'm sorry, just one final question. I'm sorry to belabour this point.

Could you just explain to me once again, I'm slow at getting it, why these five curves back on table...figure

Q. (cont'd.) three are roughly the same up until twenty years?

A. Yes. I've talked about extrapolation this morning, extrapolating from low doses to high doses, from twenty years exposure to forty years, and said these were problems we had to overcome, or these were problems we had, and we could only extrapolate from data we had to these other situations.

If we had ...we wouldn't have this problem at all if we had data on a group of people who had been exposed for forty years to, let us say, two fibers per millilitre. If we had such a group, then we would know what the health effects of that were, we would know whether what is the present hygiene standard in a number of countries, what degree of protection that offered.

But we don't have that so we've got to extrapolate. But in extrapolating we are using what data we've got, and we are using that data to estimate certain things. In this case, we are using it to estimate parameters of the dose-response relationship. If you like, the slope of the dose-response line.

Whatever model we fit, it will fit those data. It will go through those data in some way.

Now, the data we've got, if we actually look at the paper in the British Journal of Industrial Medicine, we'll find right at the end on page 109, the average followup of sixteen years, and the average cumulative dose was eighty-four fiber years per centimeter cubed.

This is the very last paragraph of the paper on page 109, just before the acknowledgements.

DR. DUPRE: What tab is that, counsel?

MR. LASKIN: Eight.

MR. LASKIN: Q. I'm sorry, Mr. Berry. Could you just run that by again?

THE WITNESS: A. Yes. Page 109, immediately before the acknowledgements, we have the average followup was sixteen years, the average cumulative exposure was eight to four fiber years per centimeter cubed. So that the average concentration was eighty-four divided by sixteen, and that is five.

Now, this graph...we're going back to the New York paper, which is at number seven, figure three...it considers a hypothetical group of men who have been exposed to five fibers for twenty years. That is quite similar to the average of the group actually observed, and because the dose-response have been fitted to these data, the dose-response curves are bound to go through the mean observation from the data. The mean observation is sixteen years at five fibers.

It's only when we come..and so that is why the first twenty years or so, the curves are identical. They are so near together that we couldn't actually plot them all.

But then afterwards we entered this extrapolation to a plot. In other words, we are getting further and further away from the data, and because we are getting further and further away from the data, we are dependent...we are becoming more and more dependent on the assumptions that have gone into the model, and these assumptions are quite different and so the curves get further and further apart.

They are bound to fit the first twenty years' data on our bridge, because we've got about twenty years of data.

The corollary of this is, if instead of talking about a lifetime's exposure, if we talked about a man who had only been exposed for twenty years, then we could estimate that, say, a lot more precisely. I think you'll find that Peto goes to that in one of his papers, but he makes precisely that point.

Q. Just while we're at that point, at that page... thanks, Mr. Berry....could you elaborate on two of the comments you are making in this concluding paragraph? The first comment

Q. (cont'd.) appears back at page 108, in the first sentence, where you say, "The results of the study are disappointing in that it is not possible to draw any definite conclusions on the effect of the present two fiber standard".

Then, over the page, just towards the end you say, "In view of these findings, there is no room for complacency about the two fiber standard".

A. Yes.

Q. Could you help us a little bit on that?

A. Yes, right. Well, I mean the first point, one does this type of study in order to give guidance on what should be done in the future, and so that's why it's disappointing to find that one can't do that. Well, at least one can give guidance, but then only within a wide range of variation. That's what I mean by definite conclusion. We can't actually say what is the health effect of two fibers for forty years. We can't say so much disease. We can only say it's either that amount or that amount, or anything in between.

But because the disease situation within this range, at the top end of this range, going back to the most pessimistic possibility, which one of the Commissioners raised, because that is a high disease rate, that is why we say there is no room for complacency. Because the actual situation could be quite a lot worse than had originally been thought.

On the other hand, it might just be a little bit worse. It could be a lot worse.

Q. Is there a percentage that you attach somewhere in this paper to two fibers, a percentage of disease or whatever index you used, at two fibers?

A. Yes. You can go back to table eight again, on page 107. The lefthand column of that gives the prevalence of disease at two fibers per centimeter cubed.

Q. Based on the various models?

A. Yes. And we've got the range from four to fourteen percent.

Q. At...

A. At forty years.

Q. At forty years.

Can I, just following up this trend, can I take you to your paper which is at tab four, on the setting of a hygiene standard?

Can I take you to the bottom of the first page, page 145, in the lower righthand corner, where you, I take it, are making the argument that in approaching the question of setting a hygiene standard there are some serious practical objections to starting from the proposition that there is a threshold?

A. That's right. Yes.

Q. Could you just elaborate on the reasons, and particularly the mathematical example that you use?

A. Yes. Perhaps I could draw a diagram here.

We'll say this is response, this is dose. Then the two may be moved forward. One is the threshold concept which says something like this, some dose which is completely safe because the defence mechanisms of the body will take care of that by some means or other, and if that's the case and if we know what this threshold is, then we will be in a very strong position because we can say if we get the dose down to this level there is no disease and nothing to worry about.

The other situation is where we say at zero dose there is no response. That seems reasonable. If you've got no asbestos, you've got no asbestos-related disease.

You've got to go there, but you then say as dose increases, disease increases. You could end up with a dose-response curve which is very similar to this blue one,

A. (cont'd.) except just down in this area.

But the disease rate here will be so low that it will be very difficult to detect it, and I talk in that paper about we have a thousand people exposed...let us say we have a thousand people exposed down here, and we observe that zero of them have got disease.

Now some people would have argued that certainly in the past they don't know what they would do, so today this means we can say there is a threshold and it's above this red cross.

But what I say there was that the...if you observe no people with disease, out of the thousand, the upper limit could be that three people have got disease. That is, in a bigger group, three out of every thousand had got disease. And by chance, a thousand that you had got didn't have disease. You know, we're just looking for a rare event. There's not very many of them are. It's a bit like drawing a lottery ticket. Your number is not going to come up very often. This is the case here.

So the actual disease incidence could be as high as three per thousand, and that might be...so we could then say if we were fitting a nonthreshold curve, that that point, the upper limit there, had to be less than...that would be 0 point three percent.

So what I was trying to say there was, that epidemiologically we can't show whether there is a threshold or not. For people who want to believe there is a threshold, then with data like this you can't say that they are wrong.

But equally, for people who say there isn't a threshold, the disease...if there is a small dose, there will be a small risk...then that can't be disproved either.

As again it gets a bit back to the point of choosing the most pessimistic one, which is the red one, no

A. (cont'd.) threshold, but I think also there are other problems which are discussed there, that even if you accepted there was a threshold, (a) you can't measure it anyway, so you never know where this point is, (b) if you set that as a control limit you would get variations about that, so get peaks of exposure which would go above it, so you would excursions above the threshold anyway, which would mean things weren't as safe as you thought.

So...and also, although I'm not a biologist and shouldn't stray into the biological field, this does seem more attractive an assumption than just saying effectively this is a magic figure where suddenly things start happening and nothing happens below that.

Q. In order to reduce the point three or the point zero zero three, I guess, I take it you have to make your population bigger and bigger and bigger?

A. Yes. If you have ten thousand and you observe nought, then the limits there will be point nought three. But it would never be zero, and of course nobody has got groups of ten thousand, or even a thousand with levels like this, and people have...the original report from Dreeson et al, 1938, from the States, where...and a threshold limit value was set from that, and of course the Americans have always called these threshold values, but if you read carefully in the introduction, I think I quote from that in this paper. They make a statement which implies they don't really believe in a threshold, but they do call them threshold limit values, and that's one reason why the British decided they preferred to call them hygiene standards. Now for other reasons they prefer to call them control limits.

But if you look at all the fine print, they really all mean the same thing.

Q. When Dr. Enterline was here last week, and again I don't want to be unfair to him, but I understood that

Q. (cont'd.) one of the propositions he advanced was that epidemiology wasn't sensitive enough to actually measure and detect excess risk at very low levels, in part because you could never get a cohort population large enough to do it. Is that a different way of saying some of the things that you are saying?

A. Yes. Yes, I would agree with that entirely, and I think it's a problem...you know, if we are still using asbestos in thirty years time, and assuming that control has been at two fibers or one fiber, then it will be a very big problem to determine whether there has been any ill effects on health because the current estimates as put forward by people like Peto, you are going to hear later, I understand, are talking about maybe the SMR for lung cancer is, at one fiber, would be about one point two five, a hundred and twenty-five. Which means that if you expected a hundred lung cancers, you would observe a hundred and twenty-five.

If you had that situation, that would just be about significant, so you would know it was a genuine case.

But you've got to actually get an observed number of lung cancers as high as a hundred, you've got to have a very big population, you've got to think of the figures I gave you this morning. There was never an observed, an expected value of one hundred lung cancers. They were all based on smaller numbers than that, but the excess was detectable because we were talking about three or fourfold times expectation. When you are talking about one point two five, then it becomes impossible unless you have got very big populations and a very long followup. One could envisage that it's unlikely that anybody is going to have that.

Q. Then you can...does it matter in terms of sensitivity of measurement, does it matter whether you choose, for example, a morbidity index as opposed to a mortality index,

Q. (cont'd.) for example? Do your measurements become more sensitive if, instead of choosing deaths as your index, you choose possible asbestosis? Can you measure at lower levels of risk?

A. Yes. You can certainly observe things sooner because asbestosis in some of these early symptoms, early signs we have been talking about, are detectable in life. Therefore, they are detectable before somebody has died, whereas in a mortality study, you are waiting for people to die. You get a higher...you get asbestosis in people who don't develop lung cancer, so you've got more of these cases. You've got more and you've got them sooner, so it is more senstive yes.

MR. LASKIN: I'm sorry, Dr. Uffen.

DR. UFFEN: No, I have a question that is quite related to what we have just been pursuing, I think.

In previous testimony, down in that low dose level, below where we have been discussing presence or absence of the thresholds, people have referred to an elbow. Are you familiar with this term? An elbow in the curve?

MR. BERRY: No, I've never heard that expression.

MR. BAZIN: What about hockey sticks?

DR. UFFEN: Well, it implies to me, when they have used the expression, that there may be a discontinuous change in the slope of the curve.

MR. BERRY: I see, yes.

DR. UFFEN: This would be down in the region where the epidemiological evidence is very difficult to establish anything.

MR. BERRY: Yes.

DR. UFFEN: I guess what I want to know is, whether you had any knowledge or views as to whether it would be possible to establish any, the presence of any such elbow, either from your own measurements of your knowledge of the literature?

MR. BERRY: No, I don't know of anything that would enable one to establish such an elbow, no. It would seem to be a sort of intermediate state between a threshold and a nonthreshold. It would be intermediate between the blue and the red, wouldn't it, and...which, so it follows from that that it would be very difficult to detect.

DR. UFFEN: A few minutes ago we were using..I used the expression 'prudent', and I think you used the expression 'pessimistic'?

MR. BERRY: Yes.

DR. UFFEN: With these data that suggest that the dose...response-dose curve may have a much steeper slope than previously thought, Dement's work...?

MR. BERRY: Yes.

DR. UFFEN: What would be the most prudent assumption down in that low level, that it went through the origin and that it was linear?

MR. BERRY: Well, I mean all these studies essentially are assuming that anyway. It's just that the slopes are so different. Dement's study has just got a much steeper slope than anybody else's.

DR. UFFEN: My concern may be nonexistent, but suppose you have a steep slope like Dement's...

MR. BERRY: Yes.

DR. UFFEN: ...right down at the bottom, and then you want to put a little elbow in, which has a rather low slope...

MR. BERRY: Oh, I see. Yes.

DR. UFFEN: In other words, instead of a continuous curve, would there be any justification for such an assumption?

MR. BERRY: Well, not that I'm aware of, but that doesn't mean that arguments couldn't be produced to

MR. BERRY: (cont'd.) justify such an argument. Not that I'm aware of.

People do produce arguments for a threshold concept, and you know, quite distinguished biologists have used this, so one is hesitant to say that it can't be justified. But it's a bit outside my line of country and generally speaking statisticians and such people prefer to fit the simplest thing, and things with elbows in are very difficult to manage. As we've seen, it's difficult to justify them from the data anyway, so that it would have to be, in my view, some quite strong biological input from some biological principle that it was beyond dispute before one could start building it in.

Q. Could we, just pursuing this, turn over the page to 146?

You talk about what is called in your article a risk limited approach, which I take it...am I correct, Mr. Berry...would involve drawing the curve through zero?

A. Yes. I was quoting from somebody else and used risk limited...

Q. Yes.

A. Yes.

Q. Then you go on and indicate there are two stages in defining an acceptable dose. First, what degree of disease or what sign or symptom to use as an index, and secondly it must be decided what incidence of this index is acceptable, and then you make the point that the two are interrelated.

A. Yes.

Q. Then you make, you indicate there are three...there may be three possible additional assumptions that you can put into that assessment which may influence the level of the acceptable incidence, I take it?

A. Yes.

Q. Have your own...the question I'm only coming to...have your own...well, the three assumptions are that the

Q. (cont'd,) population at risk is subject to regular medical surveillance; if it's practical, to remove those so identified from further exposure and (c) removal will be effective in preventing a case from progressing.

I guess insofar as your own area of expertise is concerned and insofar as your own work is concerned, to what extent are any of those three assumptions applicable when we are talking about asbestos-related diseases?

A. Right. Well, I mean certainly asbestos-exposed populations are often subject to medical surveillance, and it's often practical to remove them, so that the key is how effective that removal will be.

There hasn't been a great deal of work done on this. I mean there is in animal experiments, and I refer you to...on the list I gave you...paper number eighteen, where rats were exposed for different lengths of time and the fibrosis and the tumors that occurred thereafter were observed, and certainly rats that were removed, say after six months, developed less fibrosis and had less tumors than those that were exposed for one year. So that the exposure from six months to twelve months produced extra disease. Or in other words, removing them at six months prevented some disease that would have otherwise have occurred.

But we did also have a group that was in, exposed for two years. The difference between the one-year group and two-year group was fairly slight. This we interpreted as being because it's really a factor of latency.

But the second year of exposure in a rat's life, which is on average two and a half years, is too near to the death time to produce anything. So...and another way of looking at that is, if you take people working them and you consider removing them, some of them have already got a tumor developing and nothing is going to stop that. But others of

A. (cont'd.) them will not have got to this stage and they might get to this stage because of next year's exposure. So if you remove them, you prevent that.

There has been some epidemiological work done by Jones, in New Orleans, one of Hans Weill's colleagues, in which they looked at progression on the chest radiograph, and the people who were removed from exposure, there was less progression than those who stayed in the job.

So that removal, there is evidence removal will be effective in a group sense, but it won't be effective for all individuals.

Q. Is there any work being done currently in the United Kingdom or the British Isles concerning the effects of removal, not only in terms of asbestosis, but lung cancer and indeed any other kind of cancer?

A. I'm not aware of anything relevant there, no.

I mean another aspect, I suppose, is going back to the London study, the less-than-two-years and the more-than-two-years group. The more-than-two-years had more tumors than the less than two years.

So removal within the first two years was... reduced disease, but of course that's not very practical for managing a work force because two years is such a short time.

Q. Can I take you to the most recent paper you sent us, which is tab sixteen? I just wanted to go back to table five at the end of that, to make sure I understand what is being represented there.

You were looking at causes of death in respect of certified-asbestosis individuals?

A. Yes.

Q. Are you looking at the death certificate or are you going behind the death certificate?

A. We are actually looking at a...not actually

A. (cont'd.) at the death certificate, but what is on the files of the pneumoconiosis medical panels, which when somebody dies have to carry out an investigation into the cause of death, which will usually include a post mortem. So we've got before us the opinion of a pathologist, and that's the main ingredient in this.

Q. The second column, the second class, asbestosis plus coronary heart disease.

A. Yes.

Q. Is that meant to indicate in anyway that asbestosis may have been a contributing factor to coronary heart disease?

A. Yes. Yes. That would be what that combination would indicate. If the asbestosis wasn't considered contributory, it would have occurred later down as not associated.

Q. Whose...just as an aside...whose judgement as to whether it is contributory or not do these statistics reflect?

A. It's a judgement of a pathologist, and also the medical officers of the panel.

Q. If we...dealing again with that second classification, asbestosis plus coronary heart disease...if we had gone to the death certificate for those, for Cardiff and Swansea, those nine people, what would you see on the death certificate?

A. I don't know offhand. We didn't actually see the death certificates. The panels don't have to keep death certificates. They don't get death certificates.

Q. Then...

A. But I think...see, there would be a...because these deaths have been investigated, there would usually be a coroner's inquest. What was decided there would influence

A. (cont'd.) what appeared on the death certificate, or what it was finally coded as.

Q. Then, coming down to lung cancer and mesothelioma, the figures in brackets indicate that asbestosis is a contributing cause of death in respect of those two tumors, even though the main cause of death was the cancerous tumor?

A. Yes.

Q. Again, can you...is there any way of...do you have any information as to, then, what the death certificate might show in respect of those matters?

A. Well, I haven't seen the death certificates, so I've got no direct evidence, no. But I would expect that if there was lung cancer diagnosed, that it would appear on the death certificate.

Q. Yet at the same time there might be some... indeed it would appear very probable in most of these cases, that there would be some underlying asbestosis?

A. Oh, asbestosis would also probably be mentioned on the death certificate, yes. But it would be... the cancer would be the cause of death that was coded. It would appear in national statistics as a cancer, because cancer overrides other causes. That's one of the conventions in the International Classification of Causes of Death.

Q. Then you've got a category, other cancers, and again it would appear even in respect of those other cancers that in some cases asbestosis is contributing to the cause of death?

A. Yes.

Q. What other cancers would those be?

A. Yes, I think somebody asked this morning and I wasn't able to tell you for certain, except to say that the gastrointestinal cancers are mentioned somewhere...if I can find it...oh, yes, on page 5 A, in the London panel, eight

A. (cont'd.) of those other cancers, so we are looking at eight out of twelve were gastrointestinal cancers. Which was about sixty percent higher than expectation.

At Cardiff, there were two or three, which is two or three out of six which were gastrointestinal, and and that was a little bit less than the expectation.

So still, that means combining those two, about half of them were GI cancers.

Q. You also, just while we are on this article, you also talk about age in relation to this study?

A. Oh, age at certification, yes.

Q. That appears at table ten, I take it.

A. Yes.

Q. Can you just put in a sentence or two what the thrust or the conclusion is on age as a factor in all of this?

A. Yes. If we look at table ten, and if we look at lung cancer, then ...and if we compare for either panel, the younger than fifty-five compared with those who were fifty-five or more when first certified, the ratio of observed to expected was greater in those certified at a younger age.

So this means, of course, the younger you are, the lower your risk of lung cancer. But it means that the fact of certification is doing...it's not just simply increasing your lung cancer rate by a constant factor. That factor depends on age, and the younger you are, the bigger that factor.

Now, of course, because lung cancer rates are increasing rapidly with age, then the older people still do worse. As you see there, you've got thirty-nine...well, no perhaps they don't. They do...it comes out more clearly in Cardiff.

You've got twenty out of seventy deaths...no, sorry. I'll withdraw that. The older people don't necessarily

A. (cont'd.) do worse.

These twenty out of one seventy-three have died of lung cancer, compared with eleven out of ninety-nine. I think those are about the same percentages, near enough.

I think we were a bit surprised at this conclusion, but that's what the data showed and it was...if you look at page seven, the bottom of page seven, it's the second largest association with the excess mortality.

Q. Age?

A. Yes, age at first certification.

Q. Following behind initial disablement?

A. That's right, yes.

DR. DUPRE: While we're on this paper and the tables, Mr. Berry, might I just ask a question about table six?

Your bottom line of this table, causes observed and expected for other causes or no information.

MR. BERRY: Yes.

DR. DUPRE: Now, what was the implication of that for the number that appears under the column 'expected'? Was that an expectation of deaths from others causes only, or an expectation both of death from other causes and an expectation of no information?

MR. BERRY: It was the former. For some reason, if you look back at table five, there were twenty men for whom the cause of death, for some reason, wasn't in the files. Now, of course, in the national population there is a death certificate for everybody, so there is always some information. So that these twenty deaths, maybe some of them did have lung cancer, so I'll take your point that those figures aren't strictly comparable and the the excess which sometimes shows up, particularly for London, nought to five years in other causes or no information, that might not be a genuine excess. It's just that some of the no informations really should be

MR. BERRY: (cont'd.) down as lung cancer, or maybe mesothelioma or another cancer, yes.

DR. DUPRE: One other point, and I may be repeating a question I asked this morning. If I do and if I am, I apologize.

Did you see the...did you become aware of the causes of death, the variety of causes of death that appear, when the death was not associated with asbestos disease?

MR. BERRY: Yes. I mean, we had the cause. It was entered on these forms, and they code a variety of things and, you know, offhand I can't recall...I mean we obviously thought there was nothing of any note, or we would have reported it, but I can't recall the details.

DR. DUPRE: You don't ever recall seeing a reference to lupus erythematosus, do you?

MR. BERRY: No, I don't, but that doesn't mean that there wasn't necessarily one. I think that's...I think it's doubtful.

MR. LASKIN: Q. Can we turn to your mortality study in London, which is tabs two and six? Can I just take you to tab six for a moment, at page, the top of page fifty-six?

You may have dealt with this morning. Unfortunately...and I apologize again if I'm repeating something you already dealt with, but unfortunately we didn't put in the brief the article to which this statement refers. The statement I just wanted to ask you about was the one in the very first sentence at the top of page fifty-six, that the number of deaths for mesotheliomotumors will continue to rise for some time.

Could you elaborate on that a little bit?

THE WITNESS: A. Yes. Well, of course, that is a paper which I should say we didn't have, if I can find my copy.

A. (cont'd.) As I think I mentioned this morning, this factory closed down in 1968.

But because of the latency problem, which we have been discussing, the number of people with twenty years or longer followup from first exposure, doesn't reach a peak until 1981 to 1985. They are the people, it's not until we get to that stage that the mesothelioma rate is showing up very much.

So because these people...that the number with that length of followup doesn't reach its peak for twenty years, until 1981 to 1985, then the peak number of mesotheliomas won't reach its peak until that time.

That's more or less what we are doing in this paper. We are trying various approaches on how mesothelioma rates vary with time. As I said this morning, we obviously had to apply some assumptions which only time will show whether they are true or not, but most of the assumptions we tried gave the peak in the same period of time...just the absolute levels altered a little bit.

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MR. LASKIN: One thing I find somewhat perplexing, and I may be misstating it, but it seems to me that there are a very large number of mesotheliomas, relatively speaking, in the United Kingdom, and yet a number of the North American studies show virtually no mesotheliomas.

THE WITNESS: Well, which ---

MR. LASKIN: For example, Dr. Weill's study, as I recall it, even because he'd taken his cohort twenty years from first exposure, there were no mesotheliomas, and there were two that I think had happened at eighteen and nineteen years.

MR. BERRY: Yes. I can't recall all the details of Dr. Weill's study -- what type of asbestos were they exposed to?

MR. LASKIN: As I understand it -- and, again, I don't want to do an injustice to him, but there was one plant where there was chrysotile only, and another where there was some crocidolite.

MR. BERRY: Yes. I mean, it wouldn't surprise me if he didn't find any in the chrysotile plant, but the other one I would find a bit surprising.

If you recall, this morning, I quoted some figures from Alison McDonald, where she looked at two chrysotile plants, and she found only one mesothelioma out of, I think it was, two and a half thousand deaths.

But in another factory, using either amosite or crocidolite, she'd found, I think it was, sixteen out of fourteen hundred deaths.

MR. LASKIN: Q. Is there any estimate of the amount of crocidolite that's been used in the factories that you've studied in England, as compared to the amount of crocidolite?

THE WITNESS: A. You mean, estimates in tonnage

A. (cont'd.) or in dust levels?

Q. Well, for example, even in terms of the components in the final product that's being produced.

A. Well, I don't know what those estimates are, but I would expect it could be obtained, but I don't know it.

Q. Is the conclusion that we draw from -- a general conclusion that we draw in respect of mesotheliomas that you're certainly going to see, by far, the preponderance of it in terms of exposure to crocidolite?

A. Or amosite.

Q. Or amosite?

A. Yes.

Q. And you're not ruling out the possibility that chrysotile will produce mesothelioma, but it seems to be a great deal rarer; would that be a fair statement?

A. Yes. I mean, there have been some that occurred in the Canadian miners and millers, who, as far as is known, have not been exposed to amphibole. So that, certainly they do occur, but at a very much lower percentage of deaths than in the crocidolite or amosite groups.

MR. LASKIN: Can we take about five minutes, Mr. Chairman; would that be appropriate?

DR. DUPRE: Yes; that's certainly agreeable. Thank you.

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MR. LASKIN: Q. To make sure on the record that I understand it, and it's back to your article on hygiene standards, which is tab 4; back to your example of the thousand

Q. (cont'd.) sample people who showed no evidence of asbestosis.

How do you get from there to the statement that it can then only be stated with reasonable certainty that the risk was less than three in a thousand; is that a confidence interval?

A. Yes; that is a confidence interval, yes.

Q. At what level?

A. Uhm -- I see I don't state that; that's why you're asking. I think it's a ninety-five per cent.

Q. For those of us who are not statisticians (and I certainly include myself), at the top of that list, can you just briefly tell us how you do -- how you get to confidence levels; what you do -- or is there some table you have to consult?

A. You can do it by consulting tables; you can also, in this case, do it by a calculation, which is quite a simple one. But the effect of what you're doing is, you're imagining a much bigger population with a certain percentage of people who've got some symptoms, and you draw a thousand of these out at random and observe none with the symptom, and you say, what's the probability of doing that, given that there's P per cent actually have this symptom?

And you choose, then, the upper limits of this P per cent is a value such that the probability of choosing none is equal to -- let's see -- [sneezing] -- the probability of choosing none, given that this P per cent is greater than point nought five.

Q. That's your ninety-five per cent level?

A. Yes.

Q. Thanks.

A. Actually, that would be a one-sided -- that

A. (cont'd.) would be a ninety per cent limit, actually.

Q. To be two-sided, it would have to be two and a half per cent on either side?

A. That's right.

Q. Okay; thank you.

Can we come back to this question of mesothelioma, which we started to discuss, and I understand your statement that it would appear that crocidolite is more hazardous in terms of mesothelioma.

Can you tell me whether, from your studies and your research -- what the reason for that is; is it the type of the fibre, is it the fact there are more fibres of crocidolite?

Is there any evidence, from your own research, that casts some light on why that might be?

A. Uhm -- well, not from the things I've been directly involved in. I think the main evidence is that it's to do with fibre size, and the crocidolite fibres that tend to be distributed in the air are finer than a chrysotile.

Also, chrysotile -- the long fibres of chrysotile aren't completely straight; they've got a curl in them, which means that, aerodynamically, they behave as if they're a lot thicker than the actual fibre diameter.

And a person who's done quite a lot of work in this is my colleague Dr. Timbrell, who's a physicist.

And the finer a fibre is, the more readily it will be inhaled and retained in the lung. This is from aerodynamic considerations.

Q. And the thicker it is, the less likely that is to happen?

A. Yes.

Q. And when you say finer as opposed to thicker,

Q. (cont'd.) are we talking about the diameter of the fibre as opposed to the length of it?

A. Yes.

Q. Do you have any knowledge, one way or the other, as to whether the length of the fibre as well has some effect in terms of hazard?

A. Well, there's animal work, and the classic work was done by Stanton in the United States, where he implanted different types of material in the pleura of rats and found an association between the subsequent tumor rate and the size of these fibres; and he found that the fibres longer than about eight microns, and narrower in diameter than, I think it was, point two five, or it might have been point five, they appear to be the more hazardous. That was actually putting the tumors into the animals.

And, of course, this raised the issue of, if it's the physical properties of asbestos that are responsible for the hazard, then would other materials of similar physical size have similar hazards; and this has led to a lot of animal work, with glass fibres and various other kinds of fibres. Again, Stanton did a lot of work on this; we've done some work at the pneumoconiosis unit.

And there was one relevant reference on this that I gave you (number 20), where we looked at two types of fibreglass, and one was quite a coarse sample and the other was much finer; and the finer one produced mesotheliomas in rats.

Q. Glass fibres?

A. Glass fibres, yes; not -- they didn't produce them at the same rate as crocidolite, or even how fine chrysotiles had done, but there were, I think, four out of thirty-two rats with mesotheliomas.

And, as I say, Stanton's done a lot of work, and

A. (cont'd.) he's produced, with some samples, much higher percentages of mesotheliomas.

And until fairly recently, this has been considered only as a sort of warning, that glass fibre might be dangerous and, if it was manufactured finely enough -- and there's an indication that as it's possible, I understand, to control the size, that it shouldn't be manufactured to too fine a diameter.

But there's recently been some work from Turkey, which I think you've heard about, by Professor Barish, where he's found a village with a very high mesothelioma rate (higher rate than has occurred in this factory in London that I've been talking about), and that's thought to be due to zeolite fibres, which occur there as a result of volcanic eruption in the past; and these are -- possibly, just have the right sizes to produce this big effect.

So it has now been shown in man that non-asbestos fibres can produce mesotheliomas at, as I say, a very high rate.

DR. DUPRE: Mr. Berry, what is the number 20 to which you just referred?

MR. LASKIN: I think, to clear that up, Mr. Chairman -- unfortunately, when we selected the articles to put in the brief, which is Exhibit 13, we apparently left out some important articles that Mr. Berry had written; and he, when he came this morning -- when I met him this morning -- he indicated to me there were five or six additional articles which he thought were important, and he said he may refer to.

Now, I gather -- I'll circulate the list; I don't have the articles myself -- I gather, three or four of them have to do with animal experiments?

THE WITNESS: Yes.

MR. LASKIN: And then there's the one article that

MR. LASKIN: (cont'd.) you and Dr. Newhouse did on mesotheliomal tumors?

THE WITNESS: Yes.

MR. LASKIN: So I will undertake to circulate that list.

DR. DUPRE: Just to interject a question, do your numbers go up with the recency of the article, so is number 20, then, a very -- a relatively recent article?

MR. LASKIN: I think the most recent article on the additional list was 1976, if I'm not mistaken.

THE WITNESS: Yes; that's right, yes. Now, it doesn't necessarily indicate it's recent, because, in your brief, you tended to have got my more recent articles, but not all of the older ones.

DR. DUPRE: The reason I asked is that, under tab 5, which is your Lyon article, 1973, you reported, at page 287, on the righthand column, the findings of Stanton and Wrench, 1972, reporting the occurrence of mesotheliomas with silicon dioxide, glass-wool and fibre-glass.

Now, is this tab 20, which is now 1976, something that gives us an updating of this, and therefore more reliable information?

THE WITNESS: Well, it's just a -- it's a report of an experiment which we did at the pneumoconiosis unit, which was in the same sort of field as Stanton was looking at; although Stanton looked at it in a lot more detail than we have. We just had two glass fibres; in one of his experiments, he had seventeen different samples.

MR. LASKIN: I suppose, Mr. Chairman, to the extent that Mr. Berry has those articles available, and if, indeed, we go over till tomorrow morning or, indeed, take a break later on, I'll arrange to have them xeroxed and

MR. LASKIN: (cont'd.) distributed to the parties.

Q. Is it fair to say, Mr. Berry, that, insofar as we can rely upon this kind of evidence that you've just been telling us about, that it says that it may not necessarily be some unique chemical properties of asbestos that produce the health hazards that we have, and it may, in fact, be the size and shape of inorganic fibres?

THE WITNESS: A. Yes. Yes; that is quite a popular hypothesis. Yes.

Q. One matter that seems to be of some interest, because I have not seen it done anywhere else, is the attempt by yourself and your group to do post mortems on patients who had mesothelioma, to see what the lung tissue had by way of number of fibres.

And I wonder if we could just spend a moment or two on that. I take it that's found in your article at tab 9. I gather this was your general survey of mesotheliomas in the United Kingdom?

A. Yes.

Q. Can I take you, first of all, to page 193 of that article, figure 2.

A. Yes.

Q. And could you perhaps just explain to us what you did, and what figure 2 shows.

A. Yes. Well, what we did, this was a study that ran for one year, and we tried to get all mesiotheliomas that occurred in that year. We didn't get them all, as is discussed in the paper.

It was a study that depended on lots of pathologists sending in odd cases, and a lot of them didn't do so.

We attempted to get -- and they are the column marked 'A.' The column marked 'N' are cases where people -- the

A. (cont'd) pathologists thought they were a mesothelioma, or they might be one, but when they were submitted to the panel of pathologists making the diagnosis for this study, they were decided to be something else.

And then 'B' and 'C' are control materials; 'B' have got bronchial carinoma, and 'C' are cerebrovascular disease.

Q. What is that -- for the non-doctor here?

A. I'm a non-doctor, too -- that's a disease of the circulation system, is it not? Perhaps some medical person could explain it.

MR. LASKIN: Perhaps I could ask my -- perhaps I could ask the commissioner on the right.

THE WITNESS: I think -- I mean, you'd better get a proper medical opinion.

DR. MUSTARD: It needs to be in laymen's terms to understand it.

MR. LASKIN: Thank you, Mr. Commissioner.

THE WITNESS: Now, we attempted to get two controls for every patient with mesothelioma, but we failed to do so; again, because we were relying on pathologists who were rather busy, and a lot of them didn't see the purpose of having controls, so they didn't bother sending them in; or some of them were specialist pathologists who only saw very specialized cases; they didn't see any control -- any bronchial carcinoma, and they didn't see any cerebrovascular disease. In fact, probably a lot of people with cerebrovascular disease don't come to post mortems, so we had relatively few of those.

Now, if you look at any given set -- let's take the bottom one, for crocidolite; the lung material was then submitted to an electron microscope technique, which identifies fibres according to their chemical composition, and this was done by Dr. Pooley.

THE WITNESS: (cont'd.) And the fibres are not only identified but quantified, so we can end up with the number of fibres per milligram of lung tissue. And we've got, in the different subdivisions of this diagram, those with less than a hundred fibres per milligram, those with a hundred to a thousand, and going right to the top, a few who had over a million fibres per milligram. So, as we go up the diagram, we get an increase in amounts.

MR. LASKIN: Q. One of your control groups (control group 'B'), are those persons with bronchial carcinoma; lung cancer?

A. Yes.

Q. Why is it that they appear, those -- that group appears to have very much less fibre in the lung tissue than the people contracting a mesothelioma tumor?

A. Well, a mesothelioma is a rare condition, which is usually associated with asbestos; whereas, lung cancer is, in the United Kingdom particularly, a common cause of death in men -- about between nine and ten per cent of male deaths are lung cancer, and the majority of those are for non-occupational reasons.

Q. The control group 'B' was chosen randomly; not necessarily from asbestos-exposed workers?

A. Correct; it was chosen randomly, yes. So one would not expect there to be many asbestos workers in section 'B.'

Q. Did you, when you were looking at the amount of fibre in the lung -- did you find any relationship between the length of exposure, on the one hand -- the length of exposure to asbestos, on the one hand, and the amount of fibre in the lungs?

A. No; but in this study, we didn't have very

A. (cont'd) much data on the matter of exposure.

Q. Was that -- is that the matter that -- when you did your other mesothelioma study on the gas mask industry?

A. Yes.

Q. Tab 13. I suppose the question I'm really asking you is, did it, insofar as your data revealed it, when you looked at it, did it follow that the longer a person had been exposed to asbestos, the more asbestos fibre there would be in the lung?

A. Right. I think -- if I can just find the place -- at table 7, on page 647, is relevant to that question. Yes; and we discuss this on page 649. But there's certainly no -- you see the top part of that table, they have been exposed to crocidolite, and if you look at the amount of crocidolite in the lung, the cases, upward in order of length of exposure; those at the top have had the longest exposure (thirty-eight months), and they go down to shorter exposure.

And the one with the longest exposure does have the most dust in the lungs, but there's also one, down towards the bottom, with only six months' exposure, who's got the second largest amount; and there's no real correlation there between length of exposure and the amount of crocidolite in these lungs.

Of course, we don't know -- all we know about these women is that they were working on gas-mask production; we don't know the details of the job. And there were different operations being done, which would have different dust levels associated with them.

Q. What is the -- the column -- you've, first of all, got a column of duration in months, and the next column's crocidolite. What are the numbers under that column?

A. Well, there was a brief period in this factory when they worked with chrysotile, and then they went over

A. (cont'd) to crocidolite; so there were some women who overlapped.

Q. This is the number of months worked with crocidolite?

A. Yes; the number of months worked with crocidolite.

Q. And then the fibre content appears in the next column.

A. Yes.

Q. And there appears to be no necessary relationship between time of exposure and ultimate fibres that are found in the lungs.

A. That's right, yes.

Q. Is there any biological or medical conclusion or explanation that arises out of all of this, of which you're aware?

A. That's this lack of correlation you're speaking of?

Q. Yes.

A. Well, as I said, one factor we haven't taken into account, because we don't have the information, is the dust level -- well, the relative dust levels of different jobs -- so that we're not able to go too far with this. But no doubt there are differences in individual retention of dust as well.

And even within a given job, it's known that some workers are what are called "dirty workers" (that is, they take less care than their colleagues), and they're exposed to higher levels. So there's a lot of factors operating to destroy a relationship.

I mean, obviously, if we knew -- if we had full details, accurately, then one would expect to get a much better correlation.

Q. One final question on mesothelioma. Could I just take you to tab 14, which is the discussion summary of yours, I take it, of the Lyon conference in 1979.

A. Yes; this was a summary of a discussion; after each session, there was a discussion. And it was my job to note what went on and summarize it. And these aren't necessarily my views; it's just what was said.

Q. Can I ask you about the last paragraph on page 861: "There is strong evidence that mesotheliomas are produced by household and neighbourhood exposure."

Is that something that you've seen in your own work, or is that something indicated by others who were at the conference?

A. That's mainly indicated by others. This is a famous paper by Newhouse and Thompson which produced some of these household cases; also neighbourhood cases.

DR. MUSTARD: Are you still -- are you going to leave this subject?

MR. LASKIN: I was going to, so I ---

DR. MUSTARD: Can I just ask a question for clarification?

MR. LASKIN: Sure.

DR. MUSTARD: Am I right in interpreting -- I guess I don't know what the tab number is, but the one that you did refer to about pathology of mineral content of lungs in cases of mesothelioma in the United Kingdom in 1976.

DR. DUPRE: I believe that must be tab 9.

DR. MUSTARD: That, in effect ---

DR. DUPRE: Page 198.

DR. JUSTARD: Page 193 -- regard your B-controls with bronchial carcinoma as individuals picked at random with bronchial carcinoma and your controls of cerebrovascular disease, that there is a significant content of asbestos fibres known to

DR. MUSTARD: (cont'd) these people at death.

THE WITNESS: Yes; in some of them, indeed.

DR. MUSTARD: And, indeed, if I take a hundred fibres per milligram tissue above that as being a fair number of fibres, it would seem that, indeed, more than fifty per cent of the controls have asbestos fibres of that magnitude in the lungs.

THE WITNESS: That's right; yes.

DR. MUSTARD: Is that an indication of the amount of asbestos fibre exposure the general population may be being subjected to?

THE WITNESS: I think it must be, except for there will be some people who, by chance, happen to be asbestos workers, but asbestos workers aren't a very sizeable portion of the general population, so I would think that must be the case; yes.

DR. MUSTARD: Because it would mean that there's a fair amount of asbestos getting into the lungs, or could it mean that the technology used to determine the asbestos fibres is far from perfect; how much error was there in that? Do you know; is it foolproof?

THE WITNESS: Well, I doubt if anything's foolproof, but it was based on identifying -- well, first of all, fibres are identified by ratio; that's the definition of a fibre. And a fibre having been found, it was then subject to what's called the EDAX technique, which gives a plot-out of the mineral content, and fibres giving a certain spectrum of mineral content will be classified as crocidolite, because that is what crocidolite fibres -- how they occur, mineralogically.

Now, I'm not competent to say whether there are fibres which aren't crocidolite which could look like crocidolite by this technique, but, as far as I'm aware, that is not the case, 'cause some of these -- I should make clear that when

THE WITNESS: (cont'd) we're talking about a hundred fibres per milligram or larger figures, of course, that number of fibres haven't actually been observed. What happens is that a certain weight of lung is examined, and that gives -- and the numbers of fibres counted, because that is quick, to count the fibres. And that gives a total fibres per milligram.

Then they are -- only a certain number are then examined to identify them, and there might be one or two hundred examined. And some of these -- when we mentioned a hundred fibres per milligram, that could be, say, that one or two of the fibres out of one or two hundred were crocidolite, or what-have-you. So that some of these figures are -- they're relatively inaccurate in an absolute sense. That is why we plot these giving a tenfold -- in groups of a tenfold range of variation.

DR. MUSTARD: When you're at the 1K level, your accuracy is presumably becoming a lot better then, is it?

THE WITNESS: Yes.

DR. MUSTARD: And still you have a sizeable number of your controls showing ---

THE WITNESS: Correct, yes.

DR. MUSTARD: Has anyone else done a study, looking for asbestos fibres in the lungs of the general population?

THE WITNESS: I don't think so; not in large groups. There are a number of people throughout the world who are capable of doing this; they've got the instrumentation. But there's really only Dr. Pooley, who's been interested in doing it on large numbers of cases.

And I might add, we have done a further study, in 1977, of -- which would happen -- the source of material there was pneumoconiosis medical panel cases, and with control material chosen in a different way; and that is, we chose material from consecutive post mortems of different towns in the United

THE WITNESS: (cont'd) Kingdom, and we obtained similar results for the controls. Even in rural towns, where there's not much industry.

DR. MUSTARD: A final question, counsel. Is there any difference in the size or configuration of the fibres between the two groups?

THE WITNESS: These are fibre counts, and we don't have here data on the fibre size. Dr. Pooley has done data on fibre size, but -- and the main conclusion of his work, if I remember correctly, is that, for amphiboles, such as crocidolite, the fibre size is relatively -- covers a relatively small range; that is, they're roughly the same size, wherever they come from. But I'm quoting there from memory of his work, and it hasn't been done for all the samples in this study.

MR. LASKIN: Q. I take it he's not just counting fibres that are the fibre sizes used to set the standard; he's not just counting fibres greater than five microns in length, with a three-to-one diameter ratio?

THE WITNESS: A. No; I'm pretty certain -- well, the three-to-one aspect ratio will hold, but I'm pretty certain he's not restricting it to five microns in length. I don't know whether that's stated anywhere; probably stated in one of Dr. Pooley's papers, actually, when he discusses the method, and I think one had better refer to that to give a definite answer.

Q. Thank you, Mr. Berry.

The last topic I really wanted to discuss with you, was still on your mortality study in London, was the relationship between asbestos exposure and smoking, and can I, first of all, just take you back to tab 14, which is your discussion summary.

A. Yes.

Q. I appreciate that the statement I'm about to

Q. (cont'd) read was not one that you made, but an observation by someone named Higginson, right at the bottom of the first paragraph, where he observed, there was strong evidence that cigarette smoking acts both as an initiator and a promoter of cancer, and that the promoting effects may not be dose-dependent.

Can you help me with what that means?

A. The difference between a promoter and an initiator?

Q. Yes.

A. Well, as I understand it, an initiator is some material which can start, as the word implies -- start off a carcinogenic process; whereas, a promoter can only speed up or influence the rate of development of a tumor which has already been started by something else.

So that a promoter wouldn't produce cancer at all, unless there was something else there acting as an initiator. That may be a rather simplified version of it, but that's how I understand it.

Q. And is asbestos considered a promoter, or initiator, or both?

A. Uh -- I'm not sure; and I think it's considered to be a promoter, but I might be wrong.

Q. Do you subscribe, from your own research, to the proposition that there's a multiplier effect produced as a result of the interaction between asbestos exposure and smoking?

A. Yes; yes, indeed. And there's one paper where we go into that, which would be -- must be tab 2, I think. Yes.

And, of course, since -- the most recent evidence on asbestos and smoking was given by Hamman and his colleagues in the New York Academy of Sciences. The proceedings of that conference, 1979 -- page 473; the New York Academy of Sciences,

A. (cont'd) Volume 330, and that shows up this effect very well, indeed. If I can give you the figures ...

They have four groups; they have non-smokers with no asbestos, they have non-smokers who were exposed to asbestos, they had smokers without asbestos, and smokers with asbestos.

And the death rates they obtained for lung cancer, 11.3, 58.4, 122.6, and 601.6.

Q. What population?

A. This is -- this is the rate per hundred thousand man years.

Q. This is Hamman's work. Is this with the insulation workers' group?

A. Yes; it includes the insulators, yes, but it has other groups in to get this non-smokers, non-asbestos; so it includes information from several sources.

Of course, if you look at -- compare this with this, you've got a fivefold difference there, due to asbestos, and, similarly, here, you've got fivefold; you've got about eleven due to smoking. So you've got smoking effects, which multiplies the rates by eleven, and, in asbestos, by five, in round figures.

They discuss in this paper that it's not an additive effect. If it was an additive effect, we'd expect, in this group here -- we're relating everything to here -- we'd expect that to be about sixteen or seventeen times, or fourteen times, that group, which it clearly isn't.

They varied enough to actually call it a multiplicative effect, but it's a word they don't use, "multiplicative," but other people have used that expression, and these stated it very well.

Q. What do they call it?

A. They just call it -- I think they call it a

A. (cont'd) synergism -- yes; they call it a synergistic effect, which, of course, it is.

Q. The only other question I just wanted to ask again relates to this mortality study, and I just want to make sure I've got it clear.

At tab 6, page 55, where you talk about -- well, the paragraph just above the table; statistically significant excess mortality, and so on. The figures that you get there, is that as a result of correcting death certificates after post mortems, or ...

A. The figures -- the only correction of causes of death which have been done in this study are to classify some people as having mesotheliomas.

And even within that, within a table such as table 2, when we talk about cancers of the lung and pleura (ICD 162 and 163), observed twenty-five, that means that twenty-five of them were so classified on the death certificate.

Now, it happens that by looking at post-mortem material, we now know that four of these actually had mesotheliomas, but we didn't actually move -- then you can go to G.I. cancer; there were eight of whom we know five have got mesotheliomas -- and they were peritoneal mesotheliomas. But there are ten mesotheliomas altogether.

So that means, only one mesothelioma -- and I don't know whether it was of the pleura or the peritonium, but it doesn't matter -- which wasn't on the death certificate, wasn't down as either cancer of the lung and pleura, or of a G.I. cancer, so it was completely classified incorrectly.

But we haven't moved it down and put the cancer of lung and pleura; we haven't made that, say, twenty-six.

Q. I see. So that, generally speaking, in this study, the only thing you did correct from the death certificates

Q. (cont'd) was for mesotheliomas?

A. Yes.

Q. I suppose the only other -- I'll exceed my liberty and ask one more question, Mr. Chairman -- I suppose the only other final question I could ask you, Mr. Berry, relates to the apparent different results, in terms of excess risk or in terms of standard mortality ratios, and so on, that appears to be prevalent, depending on which branch of the asbestos industry that you happen to be in.

And can I ask you whether your own group, in your own unit, that you work with, have any suggestions or explanations as to why we're seeing different results in different aspects of the industry?

A. Well, there's -- I think the main reason we would attribute this would be to -- would be the actual process that is going on with the fibre. You take asbestos textiles; that is an operation which is quite a vigorous operation, where the chrysotile fibres could be widely dispersed. Chrysotile tends to occur in fibres which are smaller particles held together in one bundle. The more vigorous activity you do to these, then you can get these fibres, which are very thin, breaking off and becoming separate in the air, as opposed to, let us say, the friction situation, where nothing like that is happening. In fact, the fibres are being mixed with resin. It would tend to bind them more and prevent the dispersion. So that's one possible reason which seemed to be quite plausible, but it's difficult to quantify.

Q. How does the lack -- or how does the existence of dispersion, or the lack of dispersion, present a hazard?

A. Well, it would affect it because you have different fibre sizes present in the air; different distribution of fibre sizes.

Q. If it's more dispersed ...

A. You'd have more fine fibres being inhaled.

Q. I see. And it's the fine fibres that appear to cause the problem.

A. Well, not only that, but they would be more respirable; they'd be more likely to be -- to get into the lungs.

Q. Has your group uncovered any change in the physical property of asbestos fibres, as it goes through any of these processes? Is there some factor, other factor, at work changing the integrity of the fibre itself, or making it less toxic?

A. No; we haven't really done anything in that field.

Q. Any other explanations or suggestions that your group has for these differing results?

A. When we're talking about different results, are you referring to that slide I showed with Dement's study and one or two others?

Q. Yes; and, in part, your own work in various -- in differing establishments.

A. Ah, yes. Well, of course, the work, except for the brake-lining stuff, and the other one was the London factory, and, of course, there, there was amphibole asbestos as well.

So, if one is comparing operations with different fibre types, then you've got the fibre type difference. And apart from mesothelioma, there's some evidence that lung cancer is higher in amphiboles than with chrysotile. No doubt Professor Enterline discussed this, because he's produced work in that line.

MR. LASKIN: Mr. Berry, thank you very much.

MR. LASKIN: (cont'd) I think I'm finally finished, Mr. Chairman.

DR. DUPRE: Thank you very much, counsel.

Now, something -- I want to lay out two things: on the one hand, I do not want any of the parties to feel in any way rushed, in terms of the questions they wish to put to Mr. Berry; secondly, I wish to be very, very protective of Mr. Berry's time for the rest of this day, because it is now getting on to quarter to ten his time, and we certainly can meet tomorrow morning for any period of time that will be required then.

Could I just put a suggestion to the parties. If we were to meet, say, early tomorrow morning -- 8:30, for example -- and take Mr. Berry, say - well, would you, without rushing yourselves, feel that you could finish up by, say, 12:00 noon?

MR. McNAMEE: We've already canvassed it between us, and I don't think there's going to be more than about an hour and a half of questioning, if everybody's telling me .....

DR. DUPRE: It's probably safe to say that we're looking at about two hours' worth; maybe a little more.

Well, under the circumstances, Mr. Berry, I wonder if we shouldn't perhaps consider rising at this time. I'll wish you a very good night's sleep. Thank you for all you've done with us, at these ungodly hours by your time; and see you at 8:30 tomorrow morning. Would that be ---

MR. BERRY: That would be fine, yes; thank you, Mr. Chairman.

DR. DUPRE: The Commission rises until 8:30 sharp tomcrrow morning.

INQUIRY ADJOURNED

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THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

DEREK WEST